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BIOGRAPHICAL MEMOIRS

VOL. XV

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Stephen A. Forbes

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XV—FIRST MEMOIR

BIOGRAPHICAL MEMOIR

OF

STEPHEN ALFRED FORBES

1844-1930

BY

L. O. HOWARD

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1931

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STEPHEN ALFRED FORBES

1844-1930

BY L. O. HOWARD

Chronology

Born at Silver Creek, Stephenson County, Ill., May 29, 1844.
Attended the district school from school age to the age of 14.
1858-60, studied at home, under brother's instruction.
1860, attended Beloit Academy.
1861-5, soldier, U. S. Army.
1866-7, attended Rush Medical College.
1867, raised strawberries at Carbondale, Ill.
1867-9, studied and practiced medicine under a preceptor at Makanda, Ill.
1868-70, taught school and studied science at Makanda, Ill.
1870-1, taught school and studied science at Benton, Ill.
1871, studied at Illinois State Normal University.
1871-2, taught school and studied science at Mount Vernon, Ill.
1872-7, curator, museum of the Illinois State Natural History Society, at Normal, Ill.
1875-8, instructor in zoology, Illinois State Normal University.
1877, founded, and to 1917, Director, Illinois State Laboratory of Natural History.
1882-1917, State Entomologist of Illinois.
1884, Ph.D., University of Indiana.
1884-1909, professor of zoology and entomology, University of Illinois.
1886, awarded first-class medal of the Société d'Acclimatation de France, for scientific publications.
1888-1905, Dean, College of Science, University of Illinois.
1891-2, biological expeditions, Rocky Mountains, U. S. Fish Commission.
1893, special agent, U. S. Fish Commission; Director of the Aquarium, World's Columbian Exposition; Chairman, International Congress of Zoologists; President, American Association of Economic Entomologists; also pre-

pared natural history exhibit of Illinois for Columbian Exposition.

1894, established Illinois Biological Station.

1901-7, President, National Society of Horticultural Inspectors.

1905, LL.D., University of Illinois.

1908, President, Illinois Academy of Science; American Association of Economic Entomologists.

1909-21, professor of entomology, University of Illinois.

1912, President, Entomological Society of America.

1917-30, Chief, State Natural History Survey.

1918, elected member National Academy of Sciences.

Died at Urbana, Ill., March 13, 1930.

This chronology is taken from a pamphlet entitled "In Memoriam Stephen Alfred Forbes" published by the University of Illinois shortly after his death. To the chronology is added a list of the societies of which he was a member, which included among others the American Philosophical Society and the Entomological Society of France. It is also stated in the same paragraph that he was an honorary member of the American Ornithologists' Union; had been President of the Ecological Society of America, and that he was an honorary member of the International Congress of Entomology.

In the same pamphlet was printed a short article on Doctor Forbes' ancestry, education and character, written by his son Ernest Browning Forbes of the State College of Pennsylvania. He was the son of Isaac Forbes and Agnes Van Hoesen, and, racially speaking, was Scotch on his father's side and Dutch on his mother's, both theoretically and practically an admirable mixture. The first authentic record of his American ancestry is the marriage of Daniel Forbes in 1660 to Rebecca Perriman at Cambridge, Mass. There seem to be no authentic records of the origin of this Daniel or of his emigration from Scotland. From this Daniel, through Thomas, Aaron, Stephen, John and Isaac, came the subject of this sketch. Of his great grandfathers, three at least were soldiers in the Revolutionary War, namely, Stephen Forbes, who lived at Brookfield, Mass., and later at Hardwick in the same State, who served in a Hardwick company and was

given land in Vermont; Garrett Van Hoesen (on the mother's side), a sergeant in a company of sharpshooters under General Schuyler; and Captain Isaac Sawyer, a Yale student who served with distinction in the Sullivan campaign against the Iroquois in 1779.

It is Professor Ernest Forbes' opinion that among the later ancestors of his father those who left the deepest impress were his mother, who was a high-strung woman, and his paternal grandmother whose name was Anna Sawyer and who is said to have been the strongest source of the intellect of all of her descendants.

At the time of Stephen's birth at Silver Creek, Stephenson County, Illinois, the family lived in a log house on a small farm and in a condition of privation general among the pioneer families of the Middle West. His father was obliged to sell the farm upon which this log house stood, as the result of having endorsed a note for a friend, and had to begin again on a new farm, in a house built of slabs.

There were six children, namely, Flavilla Anna, born 1824, married 1842; Francis Van Hoesen, born 1828, died 1848; Mary Elizabeth, born 1831, married, 1850; Henry, born in 1833; Stephen, 1844, and Agnes Verneth, known as "Nettie," born 1845.

When Stephen was ten years old his father died. The older brother Henry was then twenty-one, but had been independent since he was fourteen, and at the time was working his way towards a college education for which he had prepared at Freeport and Rockford and on which he had made about two years' progress by private study. Stephen, his mother and Nettie were living on a farm of 140 acres, poorly equipped and heavily mortgaged, living in a cabin of a single room in which was a cookstove at one end, two beds at the other, a trundle bed under one of them, and a dining table in the middle. At one side of the building was a little "lean-to" containing a single bed on which the father of the family died. The mother was broken in health, and the two younger children were practically helpless. Henry, however, stepped into the breach. Professor Forbes has written—

"Without an hour's hesitation he gave up his personal plans, abandoned his career for which he had already given brilliant promise, and took upon his own shoulders the burden of our support and education; and from that time on for the next seven years he was not our guardian merely, but he was our guardian angel.

"He saved enough on the farm to build us a comfortable home. He taught me French, sent me to Beloit to prepare for college, and helped my sister to the Woman's College at Northwestern University at Evanston; and when the Civil War broke out he sold the farm, paid off the still existing mortgage, gave my mother the residue of the family funds to live on with her eldest daughter Flavilla, borrowed the money with which to buy his horse and mine for the cavalry service, and to the war we went together, he as lieutenant and presently captain of our company, becoming later lieutenant colonel of the regiment and colonel by brevet." (From E. B. Forbes.)

As to the details of the Army service of S. A. Forbes, I am able to quote from an unpublished autobiograpical sketch in a letter that he wrote in 1923 and which I take from the *Scientific Monthly* of May, 1930:

"My army service, concerning which you ask particulars, was begun by my enlistment as a private in Company B, 7th Illinois Cavalry, in September, 1861, when I was seventeen years old. At eighteen I was made orderly sergeant; at nineten, second lieutenant, and at twenty, captain of my company. Just after my eighteenth birthday, when sent to carry an important dispatch to a distant outpost near Corinth, Mississippi, I was put upon the wrong road and presently found myself inside the rebel lines as a prisoner of war. Telling my captors that I had a verbal message only, which I refused, and they did not compel me, to disclose, I availed myself of an opportunity to tear up my dispatch, secreting the fragments in the pistol holster on my saddle. At General Bragg's headquarters I was threatened with hanging if I did not produce my dispatch and was thoroughly searched for it, as was also my saddle, but nothing was found. Later in the day I had a brief interview with General Beauregard, in command of the rebel army after the battle of Shiloh, and a much longer one with a major of his staff, who ended by wishing me good luck, and telling me to appeal to him if I got into trouble. I was in prison four months, at Mobile, Alabama; Macon, Georgia; and Richmond, Virginia. Utilized my abundant leisure by studying Greek from books which I managed to buy at Mobile. When

paroled and released, I was sent to a hospital for three months to recover from scurvy and malaria, acquired in prison. Rejoined my regiment, reenlisted for the war, and was mustered out in November, 1865."

This is a very short and very modest account of the boy's war adventures. He was only twenty-one when he was mustered out (he had the rank of Captain at twenty), and yet he had seen nearly five years of service. The experience, apparently, hurt neither his mind nor his physique. Perhaps in the long run it was physically good for him; and the boy who used his days in a Confederate prison studying Greek was surely one whose mind was not rusting. Writing of him in *Science,* April 11, 1930, Dr. Henry B. Ward says, "It was a source of great delight to hear the story of those days, when on rare occasions he could be persuaded to relate to younger friends some of his experiences in the field."

And now as to the matter of education. In the unpublished autobiographical sketch from which we have quoted the paragraph regarding his army service, he states, "My formal education . . . was incomplete and fragmentary, partly because of the poverty of my family, my father, a pioneer farmer, having died when I was ten years old, and partly because the Civil War took me away as I was getting ready to enter college."

In the chronological list beginning this account occur the items, "Attended the district school from school age to the age of fourteen; 1858-60 studied at home under brother's instruction;[1] 1860 attended Beloit Academy."

The attendance at Beloit Academy in 1860 was a difficult one to bring about, on account of lack of money. Ernest Forbes writes in his account, "When the young Stephen was obliged to give up his studies at Beloit Academy, because the last available dollar was gone, he had to take his trunk to the railroad station in a wheelbarrow." And he has since written me, "This year at Beloit was enjoyed at the expense of real hardship for the family at home. The letters written to Stephen contained repeated

[1] Henry taught Stephen the French language, and Professor Henry B. Ward states that he studied Italian and Spanish at this time.

suggestions that he try to find a cheaper boarding place, and serious debate of his expressed desire to spend one dollar and twenty cents for a glass lamp, since his eyes suffered as a result of study by candle light."

According to S. A. Forbes' manuscript sketch, when he returned from the war he began and nearly finished a course in medicine at Rush College in Chicago. We must not be satisfied with this bare statement. In his printed account, Professor Ernest Forbes says, "He discontinued his medical studies before their completion, however, because his savings from his career as a soldier became exhausted and because a series of incidents having to do mainly with surgical operations without the use of anesthetics convinced him that he was not temperamentally adapted to medical practice." And then he goes on to write, "His interest in natural science was determined by an academic tradition in the family, by an agricultural background, by four years' out-of-doors experience in the Army, by a naturally thoughtful habit, and by a continuing scientific interest after the cessation of his medical studies."

The chronology shows that he raised strawberries at Carbondale, Ill., and taught school and studied science at Makanda and Benton, and that he studied at the Illinois State Normal University in 1871. It was about this period (in 1870) that his first notes were published, under the heading "Botanical Notes (from Southern Illinois)" in the *American Entomologist and Botanist* for September and October of that year. This journal was then edited by C. V. Riley, and each number contained a botanical section under the editorship of George Vasey then living at Richview, Ill. The notes by Professor Forbes had evidently been sent in by request. They were confined to the counties of Union and Jackson, Illinois, gave a running account of the topography of the region, and discussed the plant growth in such a way as almost to foreshadow his future interest in the broad study of environment and association that became known as ecology. At all events they secured the warm approbation of Doctor Vasey, with whom he corresponded frequently on botanical subjects.

In his unpublished autobiographical sketch, Professor Forbes, after referring to his unfinished course in medicine, goes on to say, ". . . but becoming infatuated with a study of the botany of southern Illinois I changed my plans, taught school for a living, and studied natural history as an avocation, with the result that when Major J. W. Powell, afterwards U. S. Geologist, resigned in 1872 as Curator of the Museum of the State Natural History Society at Normal, Illinois, I was chosen to succeed him there."

The old Illinois State Natural History Society had been in existence since 1858. It was chartered in 1861. Its museum was housed in the building of the State Normal School at Normal. Its final business meetings were held in Bloomington, May 26th and June 22nd, 1871, and its museum was then formally passed over to the State of Illinois to be held by the State Board of Education "for the use and benefit of the State." The earliest curator was Dr. J. A. Sewall, instructor in chemistry in the State Normal School and afterwards President of the State University of Colorado. The second curator was Major J. W. Powell, and he held this post when he made his well known western explorations. Major Powell's deputy in the Museum, who, however, served as the actual curator, was Dr. George Vasey. In 1872 Doctor Vasey was appointed Botanist of the United States Department of Agriculture in Washington and Curator of the U. S. National Herbarium under the Smithsonian Institution. He took the oath of office April 13 of that year.

Doctor Vasey (who was then a man of fifty) had a high opinion of young Forbes. They both belonged to the Natural History Society; Forbes had studied at the State Normal University in 1871, and, moreover, Doctor Vasey must have been impressed by the character of the notes Forbes had sent in for publication in the *American Entomologist and Botanist*. I take it, and in fact I have been told, that it was largely Vasey's recommendation that secured Forbes the appointment as Curator of the Museum in 1872.

Most of the facts in the last two paragraphs are taken from a long, careful and admirable address given by Professor Forbes on the occasion of a meeting called to establish the Illinois Acad-

emy of Sciences. It is entitled "History of the Former State Natural History Societies of Illinois," and was published in *Science*, December 27, 1907, pages 892-898.

For the next two or three years he was diligently occupied with his duties as curator, but found time to publish several papers, mostly relating to the study of natural history and how natural history should be taught—really educational papers. Then came the first Summer School of Natural History and his appointment as Instructor in Zoology in the Illinois State Normal University. In his 1907 address on the natural history societies of Illinois, he mentions this summer school and its effect in bringing about the organization of the second natural history society of the State. It will be interesting to quote his words:

"Under the influence of Darwin and Agassiz and Huxley, a transforming wave of progress was sweeping through college and school, a wave whose strong upward swing was a joy to those fortunate enough to ride on its crest, but which smothered miserably many an unfortunate whose feet were mired in marsh mud."

Has this movement ever been so concisely and perfectly put into words? He goes on to say,

"This wave reached central Illinois in the early seventies, with the effect to bring about in 1875 a summer school of natural history at the State Normal School—only two years, it will be noticed, after the first session of the Agassiz School at Penikese. Wilder of Cornell and W. S. Barnard, just back from Europe with a doctor's degree, were members of its teaching staff, together with Burrill of the State University, Thomas, the State Entomologist, and the present writer, who was also director of the school. Besides an abundance of living plants and animals of our own environment, we had great boxes and barrels of marine material in large variety, some of it received alive, secured by a most active collector engaged for the purpose, who scoured the New England coast for us from Portland to Buzzard's Bay."

The school was a notable success. The Philadelphia Centennial Exposition of 1876 deranged plans for its immediate continuance, but a second summer session was held in 1878 and the students organized themselves into the second of the Illinois State natural history societies. Obviously this summer helped

to turn Forbes' attention to aquatic things, for in December, '75, he published his first strictly zoological paper—on Amblystoma; and in the following year he published his "List of Illinois Crustacea with Descriptions of New Species"; and in 1877 was published the first of his papers on the food of birds, the first one on the food of fishes appearing in 1878. In 1878 also appeared the first of his strictly entomological publications, which happened to be on "Breeding Habits of Corixa" (an aquatic insect). It will thus be seen that much early work was done with aquatic forms. And during this early period he formulated plans for the starting of the State Natural History Survey which was a child of the Illinois State Laboratory of Natural History which was founded by Professor Forbes in 1877 and of which he continued to be the Director until 1917.

In his obituary notice in *Science* of April 11, 1930, Dr. Henry B. Ward says, "He will always be looked upon as the first and the leading worker in America on aquatic biology. When he started on his studies of fresh water organisms the inland waters of our country were practically unknown. He was the first man to write on the fauna of the Great Lakes, and to contribute to a knowledge of the food of fishes, a fundamental piece of work for the proper understanding of the factors concerned in solving questions that are involved in the preservation of our commercial fisheries."

It is safe to state further that he was the first man to start careful study of the food of birds, a subject that was taken up only a few years later by the old Section of Economic Ornithology started by C. Hart Merriam in the old Division of Entomology of the U. S. Department of Agriculture and that afterwards developed into the Bureau of Biological Survey.

In his work on the fauna and flora of the lakes and inland streams, he was a pioneer and was responsible for most important studies made by others. Professor C. A. Kofoid, now of the University of California, was associated with him for many years, and has sent me the following paragraph relating to this work:

"Professor Forbes early in his career took an interest in the microscopic life of fresh water. It bore fruit in his early papers

on the food of fresh water fishes. His studies in this field led to his interest in fresh water Entomostraca, an interest which led ultimately to the studies by Dr. Sharp on the Ostracoda, by his own son Dr. E. B. Forbes on the Copepoda, and by Mr. Schacht on the genus Diaptomus. His brochure of 1887, read before the Peoria Scientific Association and later reprinted with emendations, on "The Lake as a Microcosm" is a classic not only in its interpretation of a variety of lakes which he had investigated but also because it is probably one of the first if not indeed the first biological paper which recognizes the phenomenon of animal associations, the foundation of the modern science of ecology. His later work in this field was limited, by the development of his entomological interests, to explorations of the aquatic life in the waters of Yellowstone Park, of Lake Geneva, and his activity in founding and maintaining the Biological Station on the Illinois River, housed in a floating laboratory at Havana, Ill., and engaged in 1894-1900 in a continuous survey of the plankton of that stream prior to the opening of the Chicago Drainage Canal. This work resulted in my "Studies on the Plankton of the Illinois River." His interest in this work never flagged and he followed its course intimately and later in conjunction with Mr. Richardson investigated and reported on the destructive effect which the influx of sewage later had upon the life in this river."

It will be noticed in studying the bibliographical list appended to this paper that he rarely published anything about insects until early in the 1880's, his first work having been in connection with aquatic forms and with the food of fishes and birds; but in 1882, on the retirement of Cyrus Thomas, he was made State Entomologist of Illinois and two years later Professor of Zoology and Entomology in the University of Illinois. From that time on his writings naturally came to relate preponderatingly to insects.

Before we go into his work with insects, however, more must be said about his earlier work. At the memorial exercises held at the University of Illinois on March 15, 1930, an address was made by Dr. H. J. Van Cleave, a distinguished zoologist of a younger generation, who has done much work in aquatic zoology, and, rather than to attempt myself to evaluate Forbes' work at this earlier period, I will quote the words of the vastly more competent Doctor Van Cleave:

"Very early in his program, he became impressed with the significance of interrelationships between organisms and their environment. Before ecology had been conceived as an offspring of the biological sciences, Professor Forbes had adopted the ecological point of view in his published writings. As early as 1887, in a spirit of prophetic anticipation of the coming ecological era in North America, he delivered a paper on *The Lake as a Microcosm,* wherein he set forth the themes of interdependence of organisms and community of interests in aggregations of living beings. This ecological point of view has dominated his entire program of research. His contributions on insects, as well as those on fishes and on birds have been conceived in light of relationship to the environment and have always acknowledged man and human interests as essential though by no means exclusive factors of importance in the environment of organisms. As a consequence, even his economic studies have been engendered in that breadth of biological interpretation that renders them distinctive in their field.

"In his pioneer work on the food of birds, of fishes, and of insects, he contributed a wholly new method of attack upon problems in the interpretation of the economic status of animals. Many and marked have been the tributes paid him by subsequent workers in recognition of the importance of these pioneer studies.

"His early interest in the food of birds in its relation to their economic importance, found an extension in later years to a numerical study of birds in different localities. Thereby he sought a more exact means of interpreting the true value of birds in relation to agriculture.

"The long series of papers on the biology of the Illinois River, stands as one of the greatest monuments to Professor Forbes. These investigations, starting in 1894, were carried on under his leadership with the collaboration of Frank Smith, C. A. Kofoid, and R. E. Richardson, whose names will be perpetually associated with that of S. A. Forbes. Intensive work on the life of the Illinois River and correlation of physical conditions with distribution of life of the stream were carried on for a long period of years. When changed conditions made a continuation of this project seem unprofitable the researches had extended to that point where it was widely acclaimed that the Illinois River had been more thoroughly investigated as to its biology than any other river in the world.

. . . . . .

"Much of his earlier work was upon the fresh water crustacea which at the time when he began his studies were practically

unknown in this country. A colleague, C. Dwight Marsh, an internationally recognized authority in this group, has evaluated Professor Forbes' earliest taxonomic studies with the statement: 'In North America articles were published regarding some forms in the early part of the century, but nothing recognizable appeared until S. A. Forbes commenced his series of papers. Although these papers were not extensive, they were exact and carefully worked out, and to Forbes may be given the credit of laying the foundation for all subsequent work in this country.'

"Some notion of the thoroughness of his studies may be gained from the fact that a paper which he published in 1876 is the most comprehensive work on the higher crustacea of Illinois which has appeared down to the present date. When Professor Forbes was accorded that signal honor of membership in the National Academy of Sciences, the significance of this early period of his work that had escaped the memory of many of his associates was again brought to light."

It is interesting to remember that the present writer was one of the proposers of Professor Forbes for the Academy, and that he drew especial attention to the significance of the early period of his work, largely for the purpose of emphasizing his great breadth of view as a zoologist and as an entomologist.

In 1882 Forbes was made State Entomologist of Illinois. At that time only three States had had officials with this title, namely New York, Illinois and Missouri; and with the appointment of C. V. Riley in Washington in 1878, Missouri abandoned work of this kind. The Illinois position was created by the State legislature in the winter of 1866-67. The first incumbent of the office had been a very extraordinary man, Benjamin D. Walsh, an Englishman by birth, of Cambridge training, who published only one report before his death. Walsh was succeeded by Dr. William LeBaron, who published four reports as appendices to the Transactions of the State Horticultural Society from 1871 to 1874. LeBaron died in harness, and was succeeded by Rev. Cyrus Thomas, who published a series of six reports extending over the years 1875 to 1880. After the publication of the last of these reports Thomas transferred his labors to the field of ethnology, and upon his withdrawal from office Forbes, then Director of the State Laboratory of Natural History, was appointed. He held the office from that time until 1917.

In 1894 the present writer gave an address entitled "A Brief Account of the Rise and Present Condition of Official Economic Entomology." At that time Forbes had published six reports, and I wrote of them in the following words:

"Prof. Forbes' reports are among the best which have been published. They are characterized by extreme care and by originality of treatment which has seldom been equaled. The practical end is the one which he has kept mainly in view. His experiments with the arsenites against the codling moth and the plum curculio were the first careful, scientific experiments in this direction which were made, and his investigations of the bacterial diseases of insects have placed him in the front rank of investigators in this line. His monographic treatment of the insects affecting the strawberry plant is a model of its kind, and the same may be said of his work upon the corn bill-bugs and of his studies of the chinch bug. In fact, whatever insect or group of insects has been the subject of his investigations, he has attacked the problem in a thoroughly original and eminently scientific and practical manner."

I feel sure, however, that, strong as is this praise, I did not do full justice to these reports nor to Professor Forbes' work. I was afraid of superlatives and hardly appreciated his breadth of view. From the start his work made a profound impression upon every one interested in the applications of science to agriculture, and he was chosen as the second President of the Association of Economic Entomologists, following C. V. Riley who, as the original proposer of the Association and as the official government entomologist, was naturally made its first President. It is interesting to note moreover that Forbes is the only man who was ever reëlected to that office. This reëlection occurred in the year 1908. It is perfectly safe to say that all through his active career he was looked upon as the leader among the economic entomologists of America—the man who commanded every one's respect and whose judgment was considered to be absolutely sound. And a man of this type was greatly needed among the men drawn from here, there and everywhere, in the opening up of this relatively new branch of applied science. He realized from the start and as a result of his former broad studies the necessity for the careful study of all of the ecological fea-

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tures connected with the undue increase of a given species. He appreciated this before the words *ecology* and *ecological* came into use, and in a way he may be said to have been the founder of the science of ecology in the United States.

In 1917 the position of State Entomologist was merged into the Illinois Natural History Survey, and he continued as Chief of the Survey until the time of his death.

In 1884 he was called to Urbana as the head of the Department of Zoology and Entomology in the University of Illinois. The State Laboratory of Natural History was transferred to the University, and in 1917 became the Illinois Natural History Survey. His professorship of zoology and entomology in the University was changed in 1909, and he remained Professor of Entomology until his retirement as Emeritus Professor in 1921. He was Dean of the College of Science from 1888 to 1905, and after that was the Chairman of the Committee on University Educational Policy of the University Senate.

In the list of his writings, appended to this paper, will be found more than four hundred titles, but this does not include any reference to articles in fugitive journals or to any small published notes. Neither does it include the titles of such of his writings as do not concern Science. Two of them, of transcendent merit have come to my attention. His "Gierson's Cavalry Raid" was published in the Transactions of the Illinois State Historical Society, 1907, and his wonderful essay "War as an Education" appeared in the Illinois Magazine for October, 1911. Of these titles, 19 refer to educational matters, 74 to zoology aside from entomology, 10 to botany, 20 to ecology, 52 to general natural history, and 224 (a little more than half) to entomology. But this list of titles in entomology is somewhat misleading from the fact that it includes only as individual titles his reports as State Entomologist. Of these he published eighteen, and all were lengthy and included many topics of which the majority might well have appeared as separate entries in his bibliography.

All of his writings on entomological topics stand out conspicuously from the mass of publications on this subject. Here for the first time in the history of economic entomology a man not only of striking originality but of very broad biological sym-

pathies and experiences found himself engaged in the multifarious problems of insect damage to human interests. None of his predecessors or contemporaries in this field had his broad experience in biology or his broad outlook on nature. Therefore, especially in his general addresses, he establishd beyond question his leadership among all of his fellow workers. No one who heard or read his address to the Economic Entomologists at the meeting of the American Association for the Advancement of Science in Baltimore in December, 1908 (*Journal of Economic Entomology,* vol. II, no. 1, February 1909, pp. 25-35), will ever forget it. It made a very deep impression which has resulted in no end of good to all of us in our work and in our views of things.

So much for Forbes' scientific work. As to the man himself, we must devote some attention. In a charming paper on his "Ancestry, Education and Character," his son, Ernest B. Forbes, gives a very intimate insight into his father's character. It is in fact too intimate in part to be quoted here, although it was quite appropriate as a contribution at the memorial services in Urbana. I have selected the paragraphs that follow as indicating the man very clearly aside from his scientific achievements.

During the years 1861-65, his four-years' experience in the army constituted as intensive a course of instruction as any obtainable in college, and it was all education to the mind athirst and prepared to receive it. Enlisting in Company B of the Seventh Illinois Cavalry at the age of 17, he pursued his military studies as diligently as he had applied himself to books, and was made an orderly sergeant at 18, a lieutenant at 19, and captain of his company at 20. His company was on active duty during almost the whole period of the war, and he was under fire on 22 occasions including engagements at Clinton, Port Hudson, Byhalia, Moscow, Somerville, Collinsville, Okolona, Memphis, Campbellsville, Lynville, Franklin, and Nashville.

While in the vicinity of Corinth, Miss., he was captured while carrying a despatch, and spent four months in prison at Mobile, Macon, and Richmond. During this time, under desperately discouraging conditions, he maintained his morale by studying Greek; and later, after exchange, during a period of inactivity, he bought Spanish books, which he learned to read. It was all school, to the born scholar.

Of the educational value of his military experience he writes: "In one respect particularly, our experience was a hopeful prophecy, if not itself a cause, of subsequent success. Any one who had kept the solitary flame of his separate intellectual life steadily burning through all the blasts and storms of war, might reasonably believe that nothing that should happen to him thereafter could possibly extinguish it; and this, as we all know, is more than can be inferred from the completion of an ordinary college course. The eager hunger with which the students among us attacked the full tables at home, after four years or more of semi-starvation on a few husks and scraps, and the enthusiastic appreciation with which we embraced such long deferred opportunities as still remained to us, made it certain that no artificial graduation-day would put an end to our studies, and this, after all, is the best outcome of an education. Those of us who survived the Civil War in good health and strength, with morals unstained and minds still alert, have had no final cause to regret what seemed at the time the complete wreckage of our plans of life. To us war was not hell, but at the worst a kind of purgatory, from whose flames we emerged with much of the dross burned out of our characters, and with a fair chance still left to each of us to win his proper place in the life of the world."

In politics my father was a Republican, from the day when as a 14-year-old boy he listened to the Lincoln and Douglas debate at Freeport, Ill., on August 26, 1858. He came away from this historic event "quite aflame with enthusiasm for the new Republican party and especially for Lincoln as its champion, and equally incensed against Douglas as the leader and champion of the Democrats."

Of this occasion he has written, in part:

"When Lincoln arose to open the debate, my first feeling was a genuine shock of surprise, of disappointment, of chagrin at his homeliness, his awkwardness, his plainness of attire—at the farthest remove from the bearing, look, and dress of a boy's ideal; but when he began his argument in his high, penetrating voice, calm, clear, connected, and so simple and lucid that even I could follow it without effort, I got the first impression of my life of a truly lofty character and a great mind in vigorous action.

"Lincoln, as you will remember, made the opening speech at Freeport, and although his most telling points were enthusiastically applauded, there was practically nothing in the responses of his audience to indicate that there were any Democrats among them. Douglas, in fact, opened his rejoiner with a compliment, not to the speaker for his calm and persuasive speech, but to the assembly for 'the kind and respectful attention which they

yielded not only to political friends but to those opposed to them in politics.' He himself received at first the same kind of treatment, being even more frequently and vociferously applauded than Lincoln; but as he warmed up to his argument he began seemingly to try to irritate his opponents by calling them always 'black' Republicans, with an angry and contemptuous emphasis on the word 'black.' The taunt was received in silence for a few times, and then loud cries of 'white, white,' began to come from all directions, every time he used the offensive epithet, and the clamor presently became so great, after a peculiarly irritating application of it, that Douglas paused to remind his hearers 'that while Lincoln was speaking there was not a Democrat *vulgar* and *blackguard* enough to interrupt him.' It was at this point that my adhesion to republicanism became complete, and I shouted up to Douglas at the top of my boyish voice: 'Lincoln didn't use any such talk.' I was sharply reproved by those about me, and told that I must not 'talk back'; and so stood in mortified silence until Lincoln again took the stand, when he began by saying: 'The first thing I have to say to you is a word in regard to Judge Douglas' declaration about the "vulgarity and blackguardism" in the audience, . . . that no such thing as he says, was shown by any Democrat while I was speaking. Now, I only wish, by way of reply on this subject, to say that while *I* was speaking, I used no "vulgarity or blackguardism" toward any Democrat.' With this elaboration of my own sentiment I need not say that I was relieved and delighted, or that I joined in the hearty laughter and applause with which . . . his rejoinder was received."

As a Republican, however, my father was, as in most other ways, an independent, and he never hesitated to "scratch" his ticket in favor of the Democrat when he believed the Democrat to be the better man.

My father's religious status is made clear by the following, which was written in 1923: "I was, and still am, a rationalist and an agnostic, for whom what is known as faith is merely assumption, often practically necessary, since in active life one must very often act *as if* he believed what he does not and cannot really know, but unexcusable in purely theoretical matters,"—but he drew a line through the words "and still am," and wrote above them the words "as a younger man." I think that this change was dictated by a growing hope that there is more to life than a scientist knows, or can know. At this time he was much attracted by the beauty and the comfort of the orthodox beliefs of his childhood, which he had lost during his scientific career.

Throughout life my father was active in the support of liberal religion, and, with my mother, initiated the movement which resulted in the establishment of the Unitarian Church of Urbana.

My father was reticent concerning his inner and deeper life, and, in 1923, in writing of things he would do differently if he were to live again he said, "I would be open, frank, and free with respect to my theories of life, and especially of religion, instead of leaving my children, so far as I was concerned, to find their own way in these difficult and important matters, as best they individually could. It was not indifference or thoughtlessness that influenced me, but, for one thing, a belief in individual freedom in this field, unprejudiced by authority exercised upon the helpless and defenseless child, and for another a conviction that my personal views were so far out of harmony with those of the ordinary American community that my children might find themselves strangers and even outcasts among their fellows if they were to follow my lead."

Physically, my father was characteristically restless, active, and energetic. While at the height of his powers his course through the Natural History Building could be traced by the slamming of the doors behind him. The attitude of command, attained during his extensive military service, was habitual, and he expected action on the part of his subordinates.

Until the last few years he exercised with phenomenal persistence. In the early days he did a great deal of walking, running, rowing, wood-chopping, club swinging, roller-skating and horse-back riding. He rode a bicycle for many years, and averred that his use of this vehicle added ten years to his life.

Later he drove an automobile and became locally famous for a long series of minor mishaps,—resulting from the facts that the automobile came late in the period of his physical adaptability, and that he drove to the accompaniment of his intensely concentrated thinking, without knowing that he failed to give the job his whole attention.

He learned to skate, swim, and to play golf with the generation following his own, but did not become proficient in these exercises. "Common" labor was uncongenial to him, and he once said that, for him, it would be "like hitching a race-horse to a dump cart."

Mentally he was quick, acute and alert; he delighted to deal with nice distinctions, and especially with complicated situations; and had a decided predilection for statistical and graphic presentation of products of research.

He was a poor sleeper, about five hours being his normal amount. The remainder of the night he spent reading, in bed.

He was throughout life a prodigious reader, of a wide range of serious literature. When delving into treatises in the fields of philosophy, ethics, psychology, pedagogy, metaphysics, genetics, scientific theory, and the method of science, his mind was enjoying its choicest diet; and by way of lighter reading he devoured quantities of French fiction.

Temperamentally he was in all ways a man of refined sensibility. He had a developed appreciation of the dramatic; and greatly enjoyed the theatre. His soul was attuned to the poetic, and he was devoted to Browning. When deeply moved he occasionally wrote an exquisite poem—to express himself—and then, characteristically, destroyed it. He loved music, and he played the organ in the days before pianos were common. He was naturally imaginative, and by early training was deeply religious.

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Since he was of an intensely intellectual type, he was easily bored by boresome people, and made but few intimate friends. There was, however, almost always some small and choice group of kindred spirits with whom he met for feasts of philosophy, and these men he enjoyed immensely.

During his later years, as his professional interests became more perfectly organized, and more securely established, my father was able to relax a certain degree of the pressure under which he had formerly worked, and to enjoy knowing a greatly broadened group of acquaintances. These years, therefore, were increasingly rich in gratifying personal relations.

It is characteristic of the man that even among his closest friends he was fair and temperate in his expressions regarding those whom he disapproved. He was much too wise to let himself hate anyone. On occasion, however, he could demonstrate a power of description equivalent to vivisection.

The dominant motive in my father's life was overwhelmingly that of scientific research in the public service. Research was, with him, not a conscious passion, I think; it was life itself—as natural as breathing; and his public-mindedness was no less spontaneous. It is true that he managed the investment of his savings with great interest but to have been condemned to a life devoted to private gain would have been for him genuine imprisonment.

The final, supreme demonstration of his superb morale was when with health, strength, friends of his own age, and wife all gone, he was able, through sheer intellectuality, to continue at his work, stimulated by the doing of new things, in the true spirit of youth, until 9 days from the date of his death, at the age of 85 years, 9 months, and 13 days.

I have already quoted from a letter recently sent me by Prof. C. A. Kofoid that portion relating to Doctor Forbes' early work on aquatic life and animal associations, and I now introduce another passage from the same letter which refers to Professor Forbes in other ways:

Intellectually Professor Forbes was a man of rare ability marked by an acquisitive mind and an unusual perspective of values. He early mastered the languages needed in scientific research. He read widely, and no work of importance escaped his keen eye. It was his wont to glance over the incoming literature in the library of the State Laboratory of Natural History, and he often dropped a casual word to his associates on the matters which interested him or which might interest others. His library reflected very well the breadth and activity of his intellectual and scientific interests. In spite of a certain aloofness, due apparently to an inherent shyness, he enjoyed conversation on the highest planes. He sought out and cultivated the acquaintance of those with whom he found and to whom he gave intellectual stimulus. He was the leading spirit in the "Theory Club"—a group of six, consisting of Professors Kinley (later President), Palmer (Chemistry), Townsend (Mathematics), Daniels (Philosophy) and myself the youngest of the group. We met regularly in the home of some member, and digested and discussed in a heavily nicotinized atmosphere such mighty treatises as Karl Pearson's "Grammar of Science" and Ward's "Naturalism and Agnosticism." As a dialectician, Forbes was keen in analysis, alike in attack on and defense of the position of the author under discussion and of his critics among our numbers. We came to look upon him much as an arbiter. He was at his best when he was in the midst of the clarification of some knotty problem in the philosophical relations of science, when with uplifted cigar in hand he would discourse, wittily, fluently and pointedly, till his cigar went cold, on subjects far afield from Hessian fly and corn aphids but with a familiarity and readiness as masterful as though he were in a group of his entomological colleagues.

It would be inferred very naturally from this that Forbes wrote unusually well. He did more than that. He was one of the ablest writers among scientific men. I have never read anything more perfectly done than his article "War as an Education," published in the *Illinois Magazine* for October, 1911. Others have called attention to this especial ability. Henry B.

Ward says (loc. cit.): "In all his writings he manifested a beauty of style that made them unusually appealing." Herbert Osborn called him "an adept in the use of language." C. L. Metcalf says, "His writings were characterized by their remarkable, simple, lucid expression." Dr. David Kinley, in his memorial address, refers especially to Forbes' "lucidity of expression," and goes on to say, "He was a master of literary style, writing with a simplicity and clarity that would serve as a model for many specialists in literature and rhetoric."

In the long quotation from Dr. E. B. Forbes a page or two back, the paragraph beginning with the words "Of the educational value of his military experience . . ." quotes the last paragraph from "War as an Education," and it was well worth quoting. But simply for delightful reading, let me quote one more paragraph from that remarkable essay. It relates to the call of the Civil War to the youth of that time:

> I had prepared for college, and was "studying ahead" on the college course of the time as well as I could on a farm and under the tuition of an older brother, when the echo of the cannon in Charleston harbor drove all such ideas out of my mind. To remain quietly at home, busy with books and teachers while my comrades were thronging to the front to fight for *my country,* was simply impossible, and the mere thought of it intolerable. Indeed, I think that most of us were secretly glad that we had been born in the time when it was possible for a boy to do anything so wildly and gloriously different from what had been planned for him as to go to war. It was not to us a dilemma, a sacrifice; it was a privilege, an intoxicating opportunity; we could not be made to stay at home. And this was not by any means the result of our training. It was because of something born in us; it was in our blood—and it is in the blood of young Americans yet, peace gospels and Carnegie foundations to the contrary notwithstanding. The longing for adventure, the youthful spirit rising to the challenge of danger, the thrill of responsive feeling to the "call of the country," all merging in that irresistible swell of patriotic emotion which lifted a whole people on its mighty bosom—these were the influences within us and without us which made us feel for the time that war was the only thing in the world worth while.

Personally, Professor Forbes was tall (six feet or thereabouts), reasonably slender, muscular, active. People have

spoken of his "military carriage," the result of early army training. Although I had known him for more than forty years, I never noticed anything especially military in his bearing. I have just looked up a snapshot taken of him by Paul Marchal in 1913, and which is published in that famous French writer's book "Les Sciences biologiques Appliquée à l'Agriculture dans les Ètats Unis" (Figure 111), and I find that, while the figure is straight, the head is advanced, confirming my recollection of him at that time. The picture was taken on Doctor Forbes' birthday. Marchal and I happened to visit him on that day and stayed to the family birthday dinner. In earlier years he had a notably quick step and military carriage.

His son Ernest has referred to his father's physical activities and to his later interest in certain outdoor sports. Henry B. Ward, in his account in *Science*, referring to his automobile driving, says, ". . . he often chuckled at a comment on his arrest for speeding on his eightieth birthday." He was a handsome, well featured man, of, I should say, a scholarly type. A very interesting comment on his personal appearance was made by Sir William Ramsey in a letter to Doctor Kinley written after a visit to the University of Illinois. I quote two paragraphs:

When I was in Champaign, Professor Forbes, whom I saw frequently, struck me as being strangely familiar and I was always puzzled as if I ought to have recognized him. It was only after I left that the explanation occurred to me. He is the typical Aberdonian of one, and that the commonest, type. There are two distinct types of Aberdonian, one like him, and it is the most characteristic, a big, powerfully built frame of a certain characteristic build, and with a certain aspect of face. This is the type that is regularly associated with Aberdeen. If a painter were instructed to paint a picture of the battle of Harlaw, where the Aberdeen Burghers, led by Provost Davidson, defeated the whole force of the Highlands (so far as the Highlands ever could be united in anything) and were looking about for a characteristic set of five men from whom he should make up the type of Provost Davidson, Professor Forbes would certainly be one of the five.

. . . .

That is why Professor Forbes seemed to be such a familiar figure and personality. He was *the* typical Aberdonian of his

own kind, who had preserved the type absolutely unchanged. The Aberdonian never yields to circumstances but dominates them and remains himself (except in the case of a few persons who are conceited and therefore affect an Anglified accent, which only makes them comic).

In 1873 Professor Forbes married Clara Shaw Gaston, and her death in 1930 preceded his own by a few weeks only. It was a long, happy and very loving companionship. Two of Mrs. Forbes' colonial ancestors came over on the *Mayflower,* namely, John Tilley and John Howland. In her were combined French, Scotch, English and Welsh blood.

The devoted couple had five children—Bertha Van Hoesen (Mrs. Burton R. Herring), Ernest Browning, Winifred, Ethel (Mrs. Frank W. Scott), and Richard Edwin. The last-named died at the age of six years, in 1903, but all of the others are still living.

I must not conclude this account of Forbes' long and distinguished career without especial reference to an article entitled "Stephen Alfred Forbes—An Appreciation," by Frank Smith, then just retiring from his professorship in the University of Illinois, and published in *The Audubon Bulletin,* 1926, No. 17. Doctor Smith, himself a broad zoologist, gives an especially careful and appreciative survey of Forbes' work other than in entomology.

It will undoubtedly appear from what I have written that although the subject of this memoir did most important work in botany and in a number of branches of zoology, he was far more than a specialist in any branch. He studied the birds and the fishes and the insects and the life of the rivers and lakes, all as elements of a great complex, and he studied them broadly in their relations to their surroundings. Man himself was his starting ecological factor. In fact, it will be difficult if not impossible to point out a naturalist of his generation who was more original or broader or sounder.

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COMPILED BY H. C. OESTERLING, FORMER EDITOR,
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Key To Abbreviations of Titles of Illinois Publications

Bul. Sci. Assoc. Peoria, Ill....Bulletin of the Scientific Association of Peoria, Illinois.

Ill. Acad. Sci. Trans....Transactions of the Illinois State Academy of Science.

Ill. Adm. Rep....Administrative Report of the Directors of Departments of the State of Illinois.

Ill. Agr. Exp. Sta. Bul....Bulletin of the Agricultural Experiment Station of the University of Illinois.

Ill. Agr. Exp. Sta. Cir....Circular of the Agricultural Experiment Station of the University of Illinois.

Ill. Blue Book....Blue Book of the State of Illinois.

Ill. Crop Rep. Circ....Illinois Crop Report.

Ill. Dept. Agr. Trans....Transactions of the Department of Agriculture of the State of Illinois.

Ill. Ent. Exec. Rep....Executive Report of the Illinois State Entomologist.

Ill. Ent. Off. Bul....Bulletin of the Illinois State Entomologist's Office.

Ill. Ent. Off. Circ....Circular of the Illinois State Entomologist's Office.

Ill. Ent. Off. pamphlet...Pamphlet of the Illinois State Entomologist's Office.

Ill. Ent. Rep....Report of the State Entomologist on the Noxious and Beneficial Insects of the State of Illinois.

Ill. Farm. Inst. Rep....Report of the Illinois Farmers' Institute.

Ill. Fish Com. Rep....Report of the Illinois State Fish Commission.

Ill. Hort. Soc. Trans....Transactions of the Illinois State Horticultural Society.

Ill. Jour. Com....Illinois Journal of Commerce, Chicago.

Ill. Lab. Nat. Hist. Bul....Bulletin of the Illinois State Laboratory of Natural History.

Ill. Lab. Nat. Hist. Circ....Circular of the Illinois State Laboratory of Natural History.

Ill. Lab. Nat. Hist. pamphlet...Pamphlet of the Illinois State Laboratory of Natural History.

Ill. Nat. Hist. Surv. Bul....Bulletin of the Illinois State Natural History Survey.

Ill. Nat. Hist. Surv. Ent. Circ....Entomological Circular of the Illinois State Natural History Survey.

Ill. Nat. Hist. Surv. For. Circ....Forestry Circular of the Illinois State Natural History Survey.

Ill. Nat. Hist. Surv. pamphlet...Pamphlet of the Illinois State Natural History Survey.

Ill. Nat. Hist. Surv. Rep....Report on the Natural History Survey of the State of Illinois, issued by the State Laboratory of Natural History.

Ill. Sch. Jour....Illinois School Journal, Bloomington.

Ill. Sch. Rep....Report of the Superintendent of Public Instruction of the State of Illinois.

Ill. Schoolm....Illinois Schoolmaster.

Ill. Supt. Pub. Instr. Circ....Illinois Superintendent of Public Instruction Circular.

Ill. Univ. pamphlet...Pamphlet of the University of Illinois.

Ill. Univ. Quart....Quarterly and Fortnightly Notes of the Alumni Association of the University of Illinois.

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Letter of transmittal (5-9); the corn root-worm (10-31); studies on the chinch-bug—I (32-63); the strawberry crown-borer (64-75); the strawberry crown miner (76-82); the melon plant-louse (83-91); experiments with the European cabbage worm (92-97); miscellaneous notes (98-104); the food relations of predaceous beetles (105-120). Appendix: the Lombardy poplar borer, by T. J. Burrill (121-122); the Phytopti and other injurious plant mites, by H. Garman (123-143); observations on the Angoumois grain moth and its parasites, by F. M. Webster (144-154).

Report of the Director of the State Laboratory of Natural History (to the State Superintendent of Public Instruction), for the two years ending June 30, 1882. Ill. Sch. Rep., vol. 14, pp. lx-lxxi, 1883. (*Also as pamphlet,* 12 pp., 1883.)

1884

On the life-histories and immature stages of three Eumolpini. Psyche, vol. 4, pp. 123-130, pl. i, Jan.-Feb. 1884. (*See corrective note, ibid.,* vol. 4, pp. 167-168.)

Entomological notes of the season (1883). Ill. Crop Rep. Circ. 106, pp. 177-178, Mar. 1884.

Circular (on the corn plant louse and the Hessian fly). Ill. Crop Rep. Circ. 110, pp. 48-49, with 4 fig., May 1884.

The wheat-straw worm. Ill. Crop Rep. Circ. 112, pp. 14-15, with 1 fig., June 1884.

Supplementary report on insects affecting the strawberry. Miss. Vall. Hort. Soc. Trans., vol. 2, pp. 234-258, with 14 fig., 1884.

The tarnished plant bug. Minn. Hort. Soc. Rep. for 1884, pp. 339-342, 1884.

On a contagious disease of caterpillars. Ill. Hort. Soc. Trans., vol. 17, pp. 29-41, 1884.

Destruction of fish food by bladderwort (*Utricularia*). U. S. Fish Com. Bul., vol. 4, p. 443, 1884. (*Reprinted from* Forest and Stream for Sept. 4, 1884.)

Thirteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) second annual report of S. A. Forbes, for the year 1883. Ill. Dept. Agr. Trans., vol. 21, *app.*, 183 + xxi pp., 15 pl., 1884. (*Also issued separately,* May 1884.) Contents as follows:

Letter of transmittal, notes of the year, etc. (9-12); the wheat-bulb worm (13-29); the wheat-straw worm (30-38); on insects affecting sorghum and broom-corn (39-56); the black-headed grass maggot (57-59); insects injurious to the strawberry (60-180); insects injurious to the apple (181-183).

(Statement of the origin and function of the Illinois State Laboratory of Natural History.) Introduction to Ill. Lab. Nat. Hist. Bul., vol. 1, p. 2, 1884.

Report of the Director of the State Laboratory of Natural History (to the State Superintendent of Public Instruction), for the two years ending June 30, 1884. Ill. Sch. Rep., vol. 15, pp. lviii-lx, 1884.

1885

Aberration in the perch. Amer. Nat., vol. 19, p. 192, Feb. 1885.

Description of new Illinois fishes. Ill. Lab. Nat. Hist. Bul., vol. 2, pp. 135-139, March 1885.

Miscellaneous notes on farm insects. Ill. Crop Rep. Circ. 118, pp. 193-194, Mar. 1885.

The root web-worm (*Crambus zeelus* Fernald). Ill. Crop Rep. Circ. 122, pp. 38-39, with 1 fig., May 1885.

Further notes on the root web-worm. Ill. Crop Rep. Circ. 123, pp. 48-49, with 3 fig., June 1885.

On some Illinois locusts (grasshoppers). Ill. Crop Rep. Circ. 125, pp. 27-30, Aug. 1885.

[—with T. J. Burrill] Report on agricultural experiment stations. Ill. Hort. Soc. Trans., vol. 18, pp. 52-71, 1885.

A season's work in horticultural entomology. Ill. Hort. Soc. Trans., vol. 18, pp. 117-127, 1885.

(Letter to William Jackson, Mar. 8, 1884, on remedies for the cabbage worm, etc.) Ill. Hort. Soc. Trans., vol. 18, pp. 258-259, 1885.

Fourteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) third annual report of S. A. Forbes, for the year 1884. Ill. Dept. Agr. Trans., vol. 22, *app.*, vii + 136 + xi + 120 pp., 12 pl., 1885. (*Also issued separately*, Sept. 1885.) Contents as follows:

Letter of transmittal (1); entomological calendar (3-8); contributions to agricultural entomology—I, on new and little-known corn insects (11-33), —II, notes on insects injurious to wheat (34-69), —III, brief notes on sorghum insects (70-71), —IV, on some clover insects (72-74); contributions to horticultural entomology

—I, on new and imperfectly known strawberry insects (77-82), —II, on a few grape insects (83-86), —III, on new insect enemies of the blackberry and raspberry (87-92), —IV, on the speckled cutworm as a cabbage worm (93-94), —V, notes on insects injurious to the apple and pear (95-102), —VI, on some insect enemies of the soft maple (103-111), —VII, insects injurious to the elm (112-115), —VIII, brief miscellaneous notes (116-118). Appendix: general indexes to the first twelve reports . . . (prepared by ANGE V. MILLER) —introduction (v-viii); systematic list of genera and species (ix-xix); general index to insects (1-68); general index to food plants (69-94); general index to remedies, natural and artificial (95-107). Index to the fourteenth report (108-120).

A catalogue of the native fishes of Illinois. Ill. Fish Com. Rep. for 1884, pp. 60-89, with ill., 1885. (*Also, ibid.* for 1900, pp. 59-80.)

1886

Miscellaneous essays on economic entomology. Ill. Dept. Agr. Trans., vol. 23, *app.*, pp. 1-126, 1886. (*Also issued separately*, 1886.) Contents as follows:

The entomological record for 1885 (5-25); experiments on the codling-moth and curculios (26-45) [*revised reprint from* Ill. Hort. Soc. Trans., vol. 19, pp. 103-124, 1886]; a second contribution to the life history of the corn plant-louse, by H. GARMAN (46-56); partial economic bibliography of Indian corn insects (with a systematic list of corn insects), by THOMAS F. HUNT (57-126).

(Letter) to nurserymen and fruit growers of Illinois (on insects injurious to young nursery stock). Ill. Ent. Off. Circ., 1 p., Feb. 1886.

Circular (on the Hessian fly). Ill. Ent. Off. Circ., 1 p., Mar. 1886.

Circular (on the plum curculio). Ill. Ent. Off. Circ., 1 p., Apr. 1886.

Studies on the contagious diseases of insects. Ill. Lab. Nat. Hist. Bul., vol. 2, pp. 257-321, with 1 pl., June 1886. (*Reprinted, in part,* Ill. Univ. Rep., vol. 13, pp. 294-301, 1887.)

Chinch bugs in Illinois. Prairie Farmer, vol. 58, p. 491, with 3 fig., July 31, 1886.

The chinch-bug in Illinois. Ill. Ent. Off. Circ., 8 pp., Sept. 1886. (*Reprinted,* Prairie Farmer, Sept. 25 and Oct. 2, 1886.)

Notes of the past year's work. Can. Ent., vol. 18, pp. 176-177, Sept. 1886.

Leptodora in America. Amer. Nat., vol. 20, pp. 1057-1058, Dec. 1886.

A contagious disease of the European cabbage worm, *Pieris rapae*, and its economic application. Soc. Prom. Agr. Sci. Proc., vol. 7, pp. 26-32, 1886. (*Also,* Ill. Univ. Rep., vol. 13, pp. 294-301, 1887.)

Experiments on the codling moth and curculios. Ill. Hort. Soc. Trans., vol. 19, pp. 103-124, 1886. (*Revised,* Ill. Dept. Agr. Trans., vol. 23, *app.*, 26-45, 1886.)

Report of the Director of the State Laboratory of Natural History (to the State Superintendent of Public Instruction), for the year ending June 30, 1885. Ill. Sch. Rep., vol. 16, lx-lxiii, 1886.

1887

The chinch-bug in southern Illinois. Ill. Ent. Off. Circ., 2 pp., Apr. 1887. (*Also,* Prairie Farmer, vol. 59, p. 279, Apr. 30, 1887.)

(Circular of inquiry on injury to grass lands by crane-fly larvae.) Ill. Ent. Off. Circ., 1 p., Apr. 1887.

The relations of ants and aphids. Amer. Nat., vol. 21, pp. 579-580, June 1887.

(Circular letter requesting information on chinch-bug infestations.) Ill. Ent. Off. Circ., 1 p., July 1887.

Arsenical poisons for the codling moth—record and discussion of experiments for 1885 and 1886. Ill. Ent. Off. Bul., no. 1, pp. 1-24, with 11 diagrams, 1887. (*Also,* Ill. Hort. Soc. Trans., vol. 20, pp. 109-118, 1887.)

On the chinch bug in Illinois: present condition and prospects for 1887 and 1888; remedial procedure recommended. Ill. Ent. Off. Bul., no. 2, pp. 27-43, 1887.

Second contribution to a knowledge of the life history of the Hessian fly. Ill. Ent. Off. Bul., no. 3, pp. 45-61, with 1 diagram, 1887. (Advance sheets from Ill. Ent. Rep. 15, 1889.)

The lake as a microcosm. Bul. Sci. Assoc. Peoria, Ill., pp. 77-87, 1887. (*Reprinted,* Ill. Nat. Hist. Surv. Bul., vol. 15, pp. 537-550, Nov. 1925.)

(Reports as professor of zoölogy and entomology, for 1885-1886.) Ill. Univ. Rep., vol. 13, pp. 47-48, 100-102, 160-163, 1887.

(Reports of the Director of the State Laboratory of Natural History, to the Regent of the University of Illinois, 1885-1886.) Ill. Univ. Rep., vol. 13, pp. 55, 93, 100-102, 1887.

1888

On the present state of our knowledge concerning contagious insect diseases. Psyche, vol. 5, pp. 3-12 (bibliog. pp. 15-22), Jan.-Feb. 1888.

The common apple-tree borers. Ill. Ent. Off. Circ., 4 pp., 7 fig., Feb. 1888.

A new parasite of the Hessian fly. Psyche, vol. 5, pp. 39-40, with 1 fig., Apr. 1888.

Studies of the food of fresh-water fishes. Ill. Lab. Nat. Hist. Bul., vol. 2, pp. 433-473, Apr. 1888.

On the food relations of fresh-water fishes: a summary and discussion (with detailed recapitulation of data). Ill. Lab. Nat. Hist. Bul., vol. 2, pp. 475-538, July 1888.

Note on chinch-bug diseases. Psyche, vol. 5, pp. 110-111, Sept.-Oct. 1888.

(Letter on epidemic diseases of the chinch bug.) Insect Life, vol. 1, p. 113, Oct. 1888.

The Western Society of Naturalists—a presidential address. Amer. Nat., vol. 22, pp. 988-996, Nov. 1888.

Relation of wheat culture to the chinch-bug. Soc. Prom. Agr. Sci. Proc., vol. 9, pp. 27-34, 1888. (*Abstract,* Insect Life, vol. 1, pp. 222-223, Jan. 1889.)

Unsolved problems in horticultural entomology. Ill. Hort. Soc. Trans., vol. 21, pp. 92-97, 1888.

Food of the fishes of the Mississippi Valley. Amer. Fish. Soc. Trans., vol. 17, pp. 37-66, 1888.

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), as of June 6, 1887. Ill. Lab. Nat. Hist. pamphlet, 4 pp., 1888. (*See* Ill. Univ. Rep., vol. 14, p. 48, 1889.)

1889

Fifteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) fourth report of S. A. Forbes, for the years 1885 and 1886. Ill. Dept. Agr. Trans., vol. 24, *app.*, vi + 115 pp., 4 fig., 1889. (*Also issued separately,* Apr. 1890.)

Contents as follows:

Letter of transmittal (iv); the entomological record for 1885-86 (1-6); arsenical poisons for the codling moth (7-20); a second contribution to a knowledge of the life history of the Hessian fly (21-34) [*issued separately in advance,* 1887]; on the life history of the wheat bulb worm (35-39); on an outbreak of injurious locusts in central Illinois, by C. M. Weed (40-44); on some common insects injuriously affecting the foliage of young apple trees in the nursery and orchard, by C. M. Weed (45-85). Appendix: the present condition and prospects of the chinch-bug in Illinois (89-103) [*reprinted from* Ill. Ent. Off. Bul., no. 2, pp. 27-43, 1887].

Early occurrence of the chinch-bug in the Mississippi Valley. Insect Life, vol. 1, p. 249, Feb. 1889.

An early note on the periodical cicada. Insect Life, vol. 1, p. 313, Apr. 1889.

The grain plant louse in Illinois. Ill. Crop. Rep. Circ. 145, pp. 27-28, June 1889.

Arsenical poisons for the plum and peach curculio. Insect Life, vol. 2, pp. 3-7, July 1889.

Office and laboratory organization. Insect Life, vol. 2, pp. 185-187, Dec. 1889.

(Report as professor of zoology and entomology, for 1886-1887.) Ill. Univ. Rep., vol. 14, pp. 164-167, 1889.

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), as of Oct. 31, 1888. Ill. Univ. Rep., vol. 14, pp. 185-193, 1889. (*Also as pamphlet,* 10 pp., 1889.)

1890

Note on the feeding habits of *Cermatia forceps* Raf. Amer. Nat., vol. 24, pp. 81-82, Jan. 1890.

The American plum borer, *Euzophera semifuneralis* Walk. Psyche, vol. 5, pp. 295-299, 3 fig., Jan. 1890.

Sixteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) fifth report of S. A. Forbes, for the years 1887 and 1888. Ill. Dept. Agr. Trans., vol. 26, *app.*, xiii + 104 + 122 + ix pp., 6 graphs, 6 pl., 1890. (*Also issued separately*, Apr. 1890.) Contents as follows:

Letter of transmittal (vii); general record for 1887 and 1888 (ix-xiii); studies on the chinch-bug II (1-57) (*issued separately in advance*, 1889; *pp. 7-32 reprinted*, 1914); the corn bill bugs (58-77); the meadow maggots, or leather-jackets (78-83); notes on cutworms (84-97); the burrowing web-worm (98-101). Appendix: economic bibliography of the chinch-bug, 1785-1888 (1-122).

Note on an American species of Phreoryctus. Amer. Nat., vol. 24, pp. 477-478, May 1890.

The Hessian fly. Ill. Crop. Rep. Circ. 149, pp. 32-34, with 1 pl., June 1890. (*Also*, Ill. Agr. Exp. Sta. Bul. 12, pp. 377-379, with 1 pl., Nov. 1890.)

An American terrestrial leech. Amer. Nat., vol. 24, pp. 646-649, July 1890. (*Also*, Ill. Lab. Nat. Hist. Bul., vol. 3, pp. 119-122, Sept. 1890.)

On an American earthworm of the family Phreoryctidae. Ill. Lab. Nat. Hist. Bul., vol. 3, pp. 107-117, with 3 pl., Sept. 1890.

New and old insects. Ill. Hort. Soc. Trans., vol. 23, pp. 242-251, 1890.

Synopsis of recent work with arsenical insecticides. Ill. Hort. Soc. Trans., vol. 23, pp. 310-324, 1890.

Preliminary report upon the invertebrate animals inhabiting Lakes Geneva and Mendota, Wisconsin, with an account of the fish epidemic in Lake Mendota in 1884. U. S. Fish Com. Bul., vol. 8, pp. 473-487, with 3 pl., 1890.

(Letter to the Board of Direction of the Agricultural Experiment Station, proposing field experiments for protection of crops against injury by chinch bugs.) Ill. Univ. Rep., vol. 15, pp. 17-18, 1890.

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1889-1890. Ill. Univ. Rep., vol. 15, pp. 243-245, 1890. (*Also as pamphlet*, 5 pp., 1891.)

1891

On some Lake Superior entomostraca. U. S. Fish Com. Rep. for 1887-1888, pp. 701-718, with 4 pl., 1891. (*Advance sheets issued* 1890.)

A summary history of the corn-root aphis. Insect Life, vol. 3, pp. 233-238, Jan. 1891.

On the life history of the white grubs. Insect Life, vol. 3, pp. 239-246, Jan. 1891.

The fruit bark beetle. Ill. Agr. Exp. Sta. Bul. 15, pp. 469-479, with 1 pl., Feb. 1891. (*Incorporated in* Ill. Ent. Rep. 17, pp. 1-20, 1891.)

Partial list of duplicate insects in the collection of the Illinois State Laboratory of Natural History. Ill. Lab. Nat. Hist. Circ., 19 pp., May 1891.

On a bacterial insect disease. N. Amer. Practit., pp. 401-405, Sept. 1891. (*Also,* Amer. Mo. Micr. Jour., vol. 12, pp. 246-249, Nov. 1891.)

The importation of a Hessian-fly parasite from Europe. Insect Life, vol. 4, pp. 179-181, Dec. 1891.

The fruit bark beetle. Ill. Ent. Off. Bul., no. 4, pp. 63-81, with 1 pl., 1891.

Seventeenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) sixth report of S. A. Forbes for the years 1889 and 1890. Ill. Dept. Agr. Trans., vol. 28, *app.*, xv + 90 + 36 + ix pp., 8 pl., *date on title-page* 1891. (*Also issued separately,* Feb. 1892.) Contents as follows:

Letter of transmittal (vii); general record for 1889 and 1890 (ix-xv); the fruit bark beetle (1-20) [*issued as* Ill. Agr. Exp. Sta. Bul. 15, Feb. 1891]; experiments with the arsenical poisons for the plum and peach curculio (21-25); the American plum borer (26-29); on the common white grubs (30-53); additional notes on the Hessian fly (54-63); a summary history of the corn-root aphis (64-70); on a bacterial disease of the larger corn-root worm (71-73); notes on the diseases of the chinch bug (74-87). Appendix: an analytical list of the entomological writings of William LeBaron, M. D., Second State Entomologist of Illinois, compiled with the assistance of C. M. Weed, John Martin, Mary J. Snyder, and C. A. Hart. (1-36). Index to the seventeenth report (i-ix).

Horticultural entomology. Ill. Hort. Soc. Trans., vol. 24, pp. 88-95, 1891.

List of economic species of insects for public schools, with references to entomological literature. Ill. Lab. Nat. Hist. Circ., 13 pp., 1891.

The head of the English sparrow. Ill. Lab. Nat. Hist. Circ., 10 pp., 1891.

History and status of public school science work in Illinois. In "Educational Papers by Illinois Science Teachers," vol. 1, pp. 6-20, 1891.

Pedagogical contents of zoology. In "Educational Papers by Illinois Science Teachers," vol. 1, pp. 38-48, 1891.

Science in the country school. Interstate Sch. Rev. (Danville, Ill.), vol. 1, pp. 13 ff., 1891 (*not seen*).

1892

Bacteria normal to digestive organs of Hemiptera. Ill. Lab. Nat. Hist. Bul., vol. 4, pp. 1-7, Jan. 1892.

The chinch-bug in Illinois, 1891-92. Ill. Agr. Exp. Sta. Bul. 19, pp. 44-48, Feb. 1892.

Zoology in the public school: choice and arrangement of material. (Ill.) Public Sch. Jour., vol. 11, pp. 319-321, 375-377, 429-431, Mar.-May 1892.

The Illinois insectarium. Amer. Nat., vol. 26, pp. 353-354, Apr. 1892.

An all-around microscope. Amer. Mo. Micr. Jour., vol. 13, pp. 91-92, Apr. 1892.

Experiments with chinch-bugs. Psyche, vol. 6, p. 250, Apr. 1892.

Address of first vice-president (Assoc. Econ. Ent., Aug. 15, 1892, Rochester, N. Y.) on contagious diseases of insects. Insect Life, vol. 5, pp. 68-76, Nov. 1892.

(Food habits of the common yellow thrips.) Insect Life, vol. 5, pp. 126-127, Nov. 1892.

(Letter to James Fletcher, on the Natural History Hall of the University of Illinois.) Ottawa Naturalist, vol. 6, pp. 133-135, Dec. 1892.

The fruit-destroying insects of southern Illinois. Ill. Hort. Soc. Trans., vol. 25, pp. 116-123, 1892.

Answers to interrogations (on) horticultural entomology. Ill. Hort. Soc. Trans., vol. 25, pp. 258-259, 1892.

Report of the Illinois State Entomologist concerning operations under the Horticultural Inspection Act. U. S. D. A. Off. Exp. Sta. Bul. 12, pp. 1058-1060, 1892 (*not seen*).

1893

A preliminary report on the aquatic invertebrate fauna of the Yellowstone National Park, Wyoming, and of the Flathead region of Montana. U. S. Fish Com. Bul., vol. 11, pp. 207-258, with 6 pl., Apr. 1893.

(Provisional program for international congress of zoologists in connection with the Columbian Exposition at Chicago.) Amer. Nat., vol. 27, pp. 762-763, Aug. 1893.

Presidential address (Assoc. Econ. Ent., Aug. 14, 1893, Madison, Wisc.). Insect Life, vol. 6, pp. 61-70, Dec. 1893.

Injuries to fruits by twenty-five worst fruit insects. Ill. Hort. Soc. Trans., vol. 26, pp. 121-134 (*see also* pp. 199-204, with 25 fig.), 1893.

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1891-1892. Ill. Univ. Rep., vol. 16, pp. 284-288, 1893. (*Also as pamphlet,* 8 pp., 1893.)

1894

The chinch-bug in southern Illinois, 1894. Ill. Agr. Exp. Sta. Bul. 33, pp. 397-399, June 1894.

How to fight chinch bugs. Prairie Farmer, vol. 66, no. 31, p. 2, Aug. 4, 1894.

The biological station of the University of Illinois. Amer. Nat., vol. 28, pp. 723-724, 1894.

A program of practice in horticultural entomology. Ill. Hort. Soc. Trans., vol. 27, pp. 113-118, 1894.

The chinch bug in 1894. Prospect for 1895. Contagious disease experiments, etc. Ill. Ent. Off. Bul., no. 5, pp. 83-89, 1894. (*A brief statement of results published in full as* Ill. Agr. Exp. Sta. Bul. 38, 1895, *and in* Ill. Ent. Rep. 19, 1895.)

Damage to food-fish in Wabash River. U. S. 53d Congr., 2d Sess., H. R. Misc. Doc. no. 196, pp. 2-5, 1894. (*Also,* U. S. Fish Com. Rep. for 1893-1894, pp. 109-112, 1896.)

The Aquarium of the United States Fish Commission at the World's Columbian Exposition. Report of the director. U. S. Fish Com. Bul., vol. 13, pp. 143-158, 1894.

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1893-1894. Ill. Univ. Rep., vol. 17, pp. 304-323, 1894? (*Also as pamphlet,* 36 pp., 18 pl., 1894?)

1895

Eighteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) seventh report of S. A. Forbes, for the years 1891 and 1892. Ill. Dept. Agr. Trans., vol. 31, *app.,* xi + 171 + xiii pp., 15 pl., *date on title-page 1894; probably not issued until 1895.* (*Also issued separately,* Mar. 1895.) (*2d. ed.,* x + 149 + xii pp., 15 pl., 1920.) Contents as follows:

Letter of transmittal (vi); general record for 1891 and 1892 (vii-xi); a monograph of insect injuries to Indian corn—Part I (3-171), and index (i-xiii) [*reissued, in part, as* Ill. Agr. Exp. Sta. Bul. 44, May 1896].

Experiments with the muscardine disease of the chinch-bug, and with the trap and barrier method for the destruction of that insect. Ill. Agr. Exp. Sta. Bul. 38, pp. 25-85, with 7 pl. Mar. 1895. (*Incorporated in* Ill. Ent. Rep. 19, pp. 5-176, 1896.)

Preliminary note on a contagious insect disease. Science, vol. 2, pp. 375-376, Sept. 20, 1895.

1896

Nineteenth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) eighth report of S. A. Forbes, for the years 1893 and 1894. Ill. Dept. Agr. Trans., vol. 32, *app.,* 206 + 65 + v pp., 13 pl., *date on title-page 1895, not issued before Apr. 1896.* (*Also issued separately,* May 1896.) Contents as follows:

Letter of transmittal (3); experiments for the destruction of chinch-bugs as they emerge from fields of small grain at harvest [with an analytical list of American articles consulted, 1824-1894] (5-176) [*issued, in part,* as Ill. Agr. Exp. Sta. Bul. 38, Mar. 1895]; laboratory experiments with chinch-bugs (177-189); the white ant in Illinois [with a descriptive list of economic articles consulted] (190-204). Appendix: the Mediterranean flour moth (*Ephestia kuehniella* Zeller) in Europe and America, by W. G. Johnson (7-65). Index (i-v).

Insect injuries to the seed and root of Indian corn. Ill. Agr. Exp. Sta., Bul. 44, pp. 209-296, with 61 fig., May 1896. (*Extracted from* Ill. Ent. Rep. 18, pp. 3-171, 1894.)

Progress in economic entomology. Ill. Farm. Inst. Rep., vol. 1, pp. 103-112, with portrait, 1896.

Recent progress in horticultural entomology. Ill. Hort. Soc. Trans., vol. 29, pp. 137-148, 1896.

Summer opening of the biological experiment station of the University of Illinois. Ill. Univ. pamphlet, 24 pp., 11 pl., 1896.

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1895-1896, and special report of the Biological Experiment Station. Ill. Univ. Rep., vol. 18, pp. 298-326, with 20 pl., 1896? (*Also as pamphlet*, 31 pp., 20 pl., 1896?)

1897

The San Jose scale in Illinois. Ill. Agr. Exp. Sta. Bul. 48, pp. 413-428, with 2 fig., Apr. 1897. (*Cf.* Ill. Hort. Soc. Trans., vol. 30, pp. 109-124, 1897.) (*Incorporated in* Ill. Ent. Rep. 20, pp. 1-25, 1898.)

Circular notice concerning the San Jose scale and other fruit insects. Ill. Ent. Off. Circ., 4 pp., July 1897.

Agricultural methods with farm insects. Ill. Farm. Inst. Rep., vol. 2, pp. 22-25, with portrait, 1897.

Entomological work at the University (of Illinois). Ill. Farm. Inst. Rep., vol. 2, pp. 28-33, 1897.

Summer school of field biology and second summer opening of the University Biological Station: preliminary notice. Ill. Univ. pamphlet, 4 pp., 1897.

1898

Twentieth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) ninth report of S. A. Forbes, for the years 1895 and 1896. Ill. Dept. Agr. Trans., vol. 34, *app.*, vi + 112 + xxxii pp., 12 pl., *date on the title-page 1897; probably issued in 1898.* (*Also issued separately*, Mar. 1898.) Contents as follows:

Introductory note (v-vi); the San Jose scale in Illinois (1-25) [*issued as* Ill. Agr. Exp. Sta. Bul. 48, Apr. 1897]; field observations on white grubs (26-34); midsummer measures against the chinch-bug (35-44); a study of the causes of the disappearance of a chinch-bug outbreak (45-74); the spontaneous occurrence of white muscardine among chinch-bugs in 1895 (75-78); miscellaneous chinch-bug experiments (79-102); an entomological train wrecker (103-105); note on a new disease of the army worm (106-109). Appendix: the white pine Chermes, by E. L. Storment (iii-xxvi).

The method of science. Sch. & Home Educ. (Bloomington, Ill.), vol. 18, pp. 113-119, Mar. 1898. (*Reprinted as pamphlet*, 23 pp., 1898.)

The season's campaign against the San Jose and other scale insects in Illinois. Ill. Hort. Soc. Trans., vol. 31, pp. 105-119, 1898.

State control of injurious insects. Ill. Farm. Inst. Rep., vol. 3, pp. 67-71, 1898.

Summer school of biology and third summer opening of the Illinois Biological Station. Ill. Univ. pamphlet, 7 pp., 5 fig., 1898.

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1897-1898, (including reports on the Biological Station and the Summer School). Ill. Univ. Rep., vol. 19, pp. 340-354, 1898? (*Also as pamphlet,* 31 pp., 10 pl., 1898?)

1899

The Museum of Natural History. Illini (Univ. Ill.), pp. 396-400, March 31, 1899.

The San Jose scale. Ill. Ent. Off. Circ. (*in poster form*), Apr. 1899.

Recent work on the San Jose scale in Illinois. Ill. Agr. Exp. Sta. Bul. 56, pp. 241-287, with 4 pl., July 1899. (*Incorporated in* Ill. Ent. Rep. 21, pp. 1-44, 1900.

Lessons from the year's work with the San Jose scale. Ill. Hort. Soc. Trans., vol. 32, pp. 50-61, 1899.

1900

[—with C. A. Hart.] The economic entomology of the sugar beet. Ill. Agr. Exp. Sta. Bul. 60, pp. 397-532, with 97 fig. and 9 pl., Aug. 1900. (*Incorporated in* Ill. Ent. Rep. 21, pp. 49-184, 1900.)

The working of the San Jose scale law. Ill. Hort. Soc. Trans., vol. 33, pp. 150-159, 1900. (*Reprinted with new title:* annual statement of the Illinois State Entomologist, to the State Horticultural Society, concerning operations under the horticultural inspection act (as of Dec. 20, 1899). Ill. Ent. Off. pamphlet, 10 pp., 1900.)

Twenty-first Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) tenth report of S. A. Forbes. iv + 184 + xxviii pp., 9 pl., 97 fig., 1900. Contents as follows:

Introductory note (iv); recent work on the San Jose scale in Illinois (1-44) [*issued as* Ill. Agr. Exp. Sta. Bul. 56, July 1899]; the economic entomology of the sugar beet (49-184) [*issued as* Ill. Agr. Exp. Sta. Bul. 60, Aug. 1900].

Report of the Illinois State Entomologist (to the Governor) concerning operations under the horticultural inspection act, (as of Oct. 31, 1900). Ill. Ent. Off. pamphlet, 30 pp., 1900. (*Cf.* Ill. Hort. Soc. Trans., vol. 34, pp. 213-215, 1901.)

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On the principal nursery pest likely to be distributed in trade. Ill. Agr. Exp. Sta. Circ. 36, 43 pp., 33 fig., July 1901. (*Incorporated in* Ill. Ent. Rep. 22, 1903.)

Nursery inspection and orchard insecticide work in Illinois. U. S. D. A. Off. Exp. Sta. Bul. 99, pp. 173-176, 1901.

The crown gall as a nursery pest. Ill. Hort. Soc. Trans., vol. 34, pp. 136-149, 4 pl., 1901.

Annual statement concerning operations under the horticultural inspection act. Ill. Hort. Soc. Trans., vol. 34, pp. 213-215, 1901.

Insects injurious to corn. Ill. Farm. Inst. Rep., vol. 6, pp. 80-82, 1901.

The method of science and the public school. N. Y. Science Teachers' Assoc. Proc. for 1900, pp. 768-788, 1901. (*Revised reprint,* 18 pp. 1901.)

Biennial Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1899-1900. Ill. Univ. Rep., vol. 21, pp. 8-10, 1901. (*Also a revised version as pamphlet,* 12 pp., 1901.)

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Experiments with insecticides for the San Jose scale. Ill. Agr. Exp. Sta. Bul. 71, pp. 241-264, April 1902. (*Incorporated in* Ill. Ent. Rep. 22, pp. 67-90, 1903.)

Additional insecticide experiments for the San Jose scale. Ill. Agr. Exp. Sta. Bul. 72, pp. 265-268, May 1902. (*Incorporated in* Ill. Ent. Rep. 22, pp. 91-95, 1903.)

Insecticide experiments for the San Jose scale. Ill. Agr. Exp. Sta. Circ. 53, 2 pp., May 1902. (*An abstract of* Ill. Agr. Exp. Sta. Bul. 71 and 72.)

The corn bill-bugs in Illinois. Ill. Agr. Exp. Sta. Bul. 79, pp. 435-461, Oct. 1902. (*Incorporated in* Ill. Ent. Rep. 22, pp. 1-26, 1903.)

Methods and results of field insecticide work against the San Jose scale. Ill. Agr. Exp. Sta. Bul. 80, pp. 463-502, with 9 pl., Oct. 1902. (*Incorporated in* Ill. Ent. Rep. 22, pp. 27-66, 1903.)

The canker worm on apple trees and elms. Miss. Vall. Apple Growers' Assoc. Proc. for 1901-1902, pp. 38-41, 1902.

Entomological notes and inspection report for 1901. Ill. Hort. Soc. Trans., vol. 35, pp. 142-152, 1902.

Elementary biology in the public school. In "Superintendents' Round Table," (introductory discussion at DeKalb, Oct. 19, 1901, and synopsis of discussion at Chicago, Dec. 13, 1901) separate pamphlet, 20 pp., 1902.

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How to make the farm attractive to the educated. Unity, pp. 316-318, Jan. 15, 1903.

The scientific method in high school and college. Sch. Sci., vol. 3, pp. 53-66, May 1903.

Notes on the insecticide use of the gasoline blast lamp. Ill. Agr. Exp. Sta. Bul. 89, pp. 145-154, Nov. 1903.

Twenty-second Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) eleventh report of S. A. Forbes. viii + 149 + xx pp., 9 pl., 33 fig., 1903. Contents as follows:

Introductory note (viii); the corn bill-bugs in Illinois (1-26) [*issued as* Ill. Agr. Exp. Sta. Bul. 79, Oct. 1902]; methods and results of field insecticide work agaist the San Jose scale, 1899-1902

(27-66) [*issued as* Ill. Agr. Exp. Sta. Bul. 80, Oct. 1902]; experiments with lime and sulphur washes for the San Jose scale (67-90) [*issued as* Ill. Agr. Exp. Sta. Bul. 71, Apr. 1902]; experiments and observations on the use of crude petroleum and pure kerosene for the San Jose scale (91-95) [*issued as* Ill. Agr. Exp. Sta. Bul. 72, May 1902]; experiments with summer washes for the San Jose scale (96-97); on the principal nursery pests likely to be distributed in trade (98-138) [*issued as* Ill. Agr. Exp. Sta. Circ. 36, July 1901]; the canker-worm on shade and forest trees (139-144); the Colaspis root-worm (145-149).

Excellencies and defects of existing legislation for the control of insect and fungus pests. U. S. D. A., Off. Exp. Sta. Bul. 123, pp. 122-126, 1903.

Report of inspection and insecticide operations. Ill. Hort. Soc. Trans., vol. 36, pp. 120-121, 1903.

The San Jose and other scale insects. Ill. Hort. Soc. Trans., vol. 36, pp. 121-126, 1903.

Report of the Illinois State Entomologist (to the Governor) on the horticultural inspection law, (as of Feb. 1, 1903). Ill. Ent. Off. pamphlet, 30 pp., 1903. (*Cf.* Ill. Hort. Soc. Trans., vol. 35, pp. 142-152, 1902; vol. 36, pp. 120-121, 1903.)

(Report on) Entomology. Ill. Blue Book for 1903, pp. 474-475, 1903?

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Directions for the preparation of the Oregon wash. Ill. Hort. Soc. Trans., vol. 37, pp. 20-21, 1904.

Report of inspection and insecticide operations. Ill. Hort. Soc. Trans., vol. 37, pp. 229-238, 1904.

The more important insect injuries to Indian corn. Ill. Agr. Exp. Sta. Bul. 95, pp. 329-399, with 38 fig. and 5 pl., Nov. 1904. (*Incorporated in* Ill. Ent. Rep. 23, pp. 1-280, 1905.)

The economic entomology which the farmer should know. Ill. Farm. Inst. Rep., vol. 9, pp. 58-65, 1904. (*Also with new title:* The kind of economic entomology which the farmer ought to know. Ill. Ent. Off. pamphlet, 16 pp., 1904.)

The teaching of the scientific method. Nat. Educ. Assoc. Jour., vol. 43, pp. 879-888, 1904.

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), as of June 6, 1903. Ill. Univ. Rep., vol. 22, pp. 92-93, 1904.

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Practical treatment of the San Jose scale. Ill. Agr. Exp. Sta. Circ. 85, 4 pp., Jan. 1905.

Illinois River plankton. Science, vol. 21, pp. 233-234, Feb. 10, 1905.

Economic and industrial aspects of secondary school biology. Sch. Sci. Math., vol. 5, pp. 173-183, Mar. 1905.

[—with R. E. RICHARDSON.] On a new shovelnose sturgeon from the Mississippi River. Ill. Lab. Nat. Hist. Bul., vol. 7, pp. 37-44, with 4 pl., May 1905.

Field experiments and observations on insects injurious to Indian corn. Ill. Agr. Exp. Sta. Bul. 104, pp. 95-152, with 2 fig., Oct. 1905. (*Incorporated in* Ill. Ent. Rep. 24, pp. 1-58, 1908.)

(Review of) American Insects, by Vernon L. Kellog. Sch. Sci. Math., vol. 5, pp. 777-778, Dec. 1905.

Plum curculio in the apple orchard: a spraying experiment. Ill. Hort. Soc. Trans., vol. 38, pp. 91-107, with 4 pl., 1905.

Report of nursery inspection and insecticide operations. Ill. Hort. Soc. Trans., vol. 38, pp. 108-114, 1905.

(Conference on) insects injurious to corn. Ill. Farm. Inst. Rep., vol. 10, pp. 35-55, with portrait and col. pl., 1905.

The principal insects injurious to the corn plant. Ill. Farm. Inst. Rep., vol. 10, pp. 220-308, with 69 fig., 1905. (*Composed of portions of* Ill. Ent. Rep. 18 and 23.)

Twenty-third Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) twelfth report of S. A. Forbes. 273 + xxxiii pp., 8 pl., 238 fig., 1905. (*2d. ed.*, 280 + xxxviii pp., 1920). Contents as follows:

Introductory note (viii); a monograph of insect injuries to Indian corn. Part II. The more important insect injuries to Indian corn (1-280) [*issued as* Ill. Agr. Exp. Sta. Bul. 95, Nov. 1904].

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1903-1905. Ill. Ent. Off. pamphlet, 35 pp., 1905. (*Cf.* Ill. Hort. Soc. Trans., vol. 37, pp. 229-238, 1904; vol. 38, pp. 108-114, 1905.)

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Comparative experiments with various insecticides for the San Jose scale. Ill. Agr. Exp. Sta. Bul. 107, pp. 243-261, Apr. 1906. (*Incorporated in* Ill. Ent. Rep. 24, pp. 59-77, 1908.)

Spraying apples for the plum curculio. Ill. Agr. Exp. Sta. Bul. 108, pp. 265-286, with 3 diagrams and 4 pl., May 1906. (*Incorporated in* Ill. Ent. Rep. 24, pp. 78-99, 1908.)

The corn root-aphis and its attendant ant. U. S. D. A. Bur. Ent. Bul. 60, pp. 29-39, Sept. 1906.

Report of nursery inspection and insecticide operations. Ill. Hort. Soc. Trans., vol. 39, pp. 132-135, 1906.

Fifty years' progress in the practical control of the insect and fungus pests of Illinois horticulture. Ill. Hort. Soc. Trans., vol. 39, pp. 219-227, 1906.

(Report on) Entomology. Ill. Blue Book for 1905, pp. 528-529, 1906.

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The cottony maple scale in Illinois. Ill. Agr. Exp. Sta. Bul. 112, pp. 343-360, with 8 fig. and 3 pl., Jan. 1907. (*Incorporated in* Ill. Ent. Rep. 24, pp. 100-117, 1908.)

On the local distribution of certain Illinois fishes: an essay in statistical ecology. Ill. Lab. Nat. Hist. Bul., vol. 7, pp. 273-303, with 15 maps and 9 pl., April 1907.

An ornithological cross-section of Illinois in autumn. Ill. Lab. Nat. Hist. Bul., vol. 7, pp. 305-335, April 1907. (*2d. ed.*, pp. 305-332, 1914.)

On the life history, habits, and economic relations of the white-grubs and May-beetles. Ill. Agr. Exp. Sta. Bul. 116, pp. 447-480, with 1 fig., Aug. 1907. (*Incorporated in* Ill. Ent. Rep. 24, pp. 135-168, 1908.)

Report of nursery inspection and orchard insecticide operation. Ill. Hort. Soc. Trans., vol. 40, pp. 124-128, 1907.

Insects in relation to health. Ill. Farm. Inst. Rep., vol. 12, pp. 263-270, with portrait, 1907.

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1905-1906. Ill. Ent. Off. pamphlet, 27 pp., 1907. (*Cf.* Ill. Hort. Soc. Trans., vol. 39, pp. 132-135, 1906; vol. 40, pp. 124-128, 1907.)

History of the former state natural history societies of Illinois. Science, vol. 26, pp. 892-898, Dec. 27, 1907. (*Also* Ill. Acad. Sci. Trans., vol. 1, pp. 18-30, 1908.)

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Practical treatment for the San Jose scale. Ill. Ent. Off. Circ., 2 pp., Feb. 1908.

Experiments with repellents against the corn root-aphis. Jour. Econ. Ent., vol. 1, pp. 81-83, Apr. 1908. (*Abstract of* Ill. Agr. Exp. Sta. Bul. 130, Dec. 1908.)

Twenty-fourth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) thirteenth report of S. A. Forbes. 168 + xvi pp., 8 fig., 11 pl., 1908. Contents as follows:

Introductory note (vi), injury to corn by the timothy bill-bugs (1-7), field experiments on the corn root-aphis (8-29), field experiments for the protection of corn against chinch-bug injury (30-58) [*first 58 pp. issued as* Ill. Agr. Exp. Sta. Bul. 104, Oct. 1905]; comparative experiments with various insecticides for the San Jose scale (59-77) [*issued as* Ill. Agr. Exp. Sta. Bul. 107, Apr. 1906]; spraying apples for the plum-curculio (78-99) [*issued as* Ill. Agr. Exp. Sta. Bul. 108, May 1906]; the cottony maple scale in Illinois (100-117) [*issued as* Ill. Agr. Exp. Bul. 112, Jan. 1907]; the elm twig-girdler (118-134); on the life history, habits, and economic relations of the white-grubs and May-beetles (135-168) [*issued as* Ill. Agr. Exp. Sta. Bul. 116, Aug. 1907].

Directions for the preparation and use of the lime and sulphur wash. Ill. Hort. Soc. Trans., vol. 41, pp. 21-22, 1908. (*Also, ibid.,* vol. 42, pp. 25-27, 1909; vol. 43, pp. 29-30, 1910.)

Report of nursery inspection and orchard insecticide operations. Ill. Hort. Soc. Trans., vol. 41, pp. 196-201, 1908.

(Abstract of) a statistical study of midsummer birds of Illinois. Science, vol. 27, pp. 918-920, June 12, 1908.

The midsummer bird life of Illinois: a statistical study. Amer. Nat., vol. 42, pp. 505-519, Aug. 1908. (*Reprinted,* Ill. Lab. Nat. Hist. Bul., vol. 9, pp. 373-385, Jan. 1913.)

Experiments with repellents against the corn root-aphis, 1905-1906. Ill. Agr. Exp. Sta. Bul. 130, pp. 3-28, Dec. 1908. (*Incorporated in* Ill. Ent. Rep. 25, pp. 1-26, 1909.)

Habits and behavior of the corn-field ant, *Lasius niger americanus.* Ill. Agr. Exp. Sta. Bul. 131, pp. 31-45, with 1 pl., Dec. 1908. (*Incorporated in* Ill. Ent. Rep. 25, pp. 27-40, 1909.)

Reports of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1906 and 1907. Ill. Univ. Rep., vol. 24, pp. 29-30, 157-158, 393-394, 1908.

(Report on) Entomology. Ill. Blue Book for 1907, pp. 493-494, 1908.

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On the general and interior distribution of Illinois fishes. Ill. Lab. Nat. Hist. Bul., vol. 8, pp. 381-437, with 102 maps, Feb. 1909. (*Also in* "The Fishes of Illinois," Ill. Nat. Hist. Surv. Rep., vol. 3, pp. lxxii-cxxii, 1909.)

[—with R. E. Richardson.] The fishes of Illinois. Ill. Nat. Hist. Surv. Rep., vol. 3, cxxxi + 357 pp., 56 pl., 76 text fig.; 103 maps in a separate atlas; 1909. (*2d. ed.,* with 68 pl., 1920.)

[—with A. O. Gross.] The economic values of some common Illinois birds. Ill. Supt. Pub. Instr. Circ. 32, "Arbor and Bird Day," pp. 53-74, with 20 fig., 1909.

Aspects of progress in economic entomology. Jour. Econ. Ent., vol. 2, pp. 25-35, Feb. 1909.

Program of cooperative experiments by the farmer and the entomologist. Ill. Ent. Off. Circ., 4 pp., Mar. 1909.

The general entomological ecology of the Indian corn plant. Amer. Nat., vol. 43, pp. 286-301, May 1909. (*Reprinted,* Ill. Nat. Hist. Surv. Bul., vol. 16, pp. 447-457, Apr. 1927.)

Report on nursery inspection and orchard insecticide operations. Ill. Hort. Soc. Trans., vol. 42, pp. 63-66, 1909.

Cooperation between the farmer and the entomologist. Ill. Farm. Inst. Rep., vol. 14, pp. 128-135, 1909.

Twenty-fifth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) fourteenth report of S. A. Forbes. 124 + xxiii pp., 35 fig., 3 pl., 1909. Contents as follows: Introduction (v-xi); experiments with repellents against the corn root-aphis, 1905 and 1906 (1-26) [*issued as* Ill. Agr. Exp. Sta. Bul. 130, Dec. 1908]; habits and behavior of the corn-field ant, *Lasius niger americanus* (27-40) [*issued as* Ill. Agr. Exp. Sta. Bul. 131, Dec. 1908]; the insect pests of clover and alfalfa, by J. W. Folsom (41-124) [*issued as* Ill. Agr. Exp. Sta. Bul. 134, Apr. 1909].

Introduction (to) contents and index of the reports of the State Entomologist of Illinois, XIII-XXIV, 1884-1908, pp. 5-7, 1909.

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1907-1908. Ill. Ent. Off. pamphlet, 21 pp., 1909. (*Cf.* Ill. Hort. Soc, Trans., vol. 41, pp. 196-201, 1908; vol. 42, pp. 63-66, 1909.)

The Illinois State Laboratory of Natural History and the Illinois State Entomologist's Office. Ill. Acad. Sci. Trans., vol. 2, pp. 54-67, 1909.

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Report on nursery inspection and orchard insecticide operations. Ill. Hort. Soc. Trans., vol. 43, pp. 93-96, 1910.

The Brown-tail and gipsy moths. Ill. Hort. Soc. Trans., vol. 43, pp. 96-101, with 2 pl., 1910.

Recent work of the entomologist's office. Ill. Farm. Inst. Rep., vol. 15, pp. 140-150, with portrait, 1910.

The Hessian fly in Illinois. Ill. Agr. Exp. Sta. Circ. 146, 4 pp., Sept. 1910. (*Revised ed.*, 5 pp., June 1914.)

(Review) Catalogue of Canadian birds, by John Macoun. Science, vol. 32, pp. 343-344, Sept. 9, 1910.

Biological investigations on the Illinois River. Ill. Lab. Nat. Hist. pamphlet, 14 pp., 11 pl., 1910. *Includes:* I. The work of the Illinois Biological Station. (*Abstract*, Science, vol. 31, pp. 837-838, May 27, 1910.) II. The investigation of a river system in the interest of its fisheries. (*Cf.* Amer. Fish. Soc. Trans., vol. 40, pp. 179-193, with 2 pl., 1911).

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1909-1910. Ill. Ent. Off. pamphlet, 11 pp., 1910. (*Cf.* Ill. Hort. Soc. Trans., vol. 43, pp. 93-96, 1910; vol. 44, pp. 90-93, 1911.)

Relations of the Illinois Academy of Science to the State. Ill. Acad. Sci. Trans., vol. 3, pp. 32-43, 1910.

[—with others.] Report of committee on an ecological survey (of Illinois). Ill. Acad. Sci. Trans., vol. 3, pp. 51-56, 1910.

1911

The chinch bug abroad again in Illinois. Ill. Ent. Off. Circ., 4 pp., May 1911.

Household pests and their relation to public health. (Chicago) City Club Bul., vol. 4, pp. 77-80, May 1911.

Economic entomology (abstract of an address). Ill. Farm. Inst. Rep., vol. 16, pp. 309-316, 1911.

Report on nursery inspection and orchard insecticide operations. Ill. Hort. Soc. Trans., vol. 44, pp. 90-93, 1911.

Forestry survey of Illinois. Ill. Hort. Soc. Trans., vol. 44, pp. 93-96, 1911.

Some important insects of Illinois shade trees and shrubs. Ill. Agr. Exp. Sta. Bul. 151, pp. 463-529, with 67 fig., Oct. 1911. (*Incorporated in* Ill. Ent. Rep. 26, pp. 1-67, 1911.) (*Reprinted*, Ill. Farm. Inst. Rep., vol. 21, pp. 244-294, 1916.)

The chinch-bug situation in Illinois: fall and winter measures to be taken. Ill. Agr. Exp. Sta. Circ. (*without serial number*), 4 pp., Nov. 1911. (*Reprinted*, Ill. Farm. Inst. Rep., vol. 17, pp. 300-302, 1912.)

[—with others.] (Report of the) committee on ecological survey (of Illinois). Ill. Acad. Sci. Trans., vol. 4, pp. 24-27, 1911.

J. A. West: in memoriam. Ill. Acad. Sci. Trans., vol. 4, p. 158, 1911.

Twenty-sixth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) fifteenth report of S. A. Forbes. 160 + xix pp., 133 fig., 1911. Contents as follows:

Some important insects of Illinois shade trees and shrubs (1-67) [*issued as* Ill. Agr. Exp. Sta. Bul. 151, Oct. 1911, *and reprinted in* Ill. Farm. Inst. Rep., vol. 21, pp. 244-294, 1916]; miscellaneous economic insects, by CHARLES A. HART (68-98); on the more important insects of the truck-farm and vegetable garden, by J. J. DAVIS (99-160) [*also in* Ill. Farm. Inst. Rep., vol. 16, pp. 216-263, 1911].

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), as of June 3, 1909. Ill. Univ. Rep., vol. 25, pp. 200-201, 1911.

Illinois Biological Station. Internat. Rev. Hydrobiol., vol. 4, pp. 226-227, with 1 pl., 1911.

1912

Definite results of survey work on the Illinois River. Amer. Fish. Soc. Trans., vol. 41, pp. 75-89, with 1 map, 1912. (*Revised version, with new title:* Chemical and biological investigations on the Illinois River, midsummer of 1911. Ill. Lab. Nat. Hist. pamphlet, 9 pp., 1 map, 1912.)

What is the matter with the elms in Illinois? Ill. Agr. Exp. Sta. Bul. 154, pp. 3-22, with 4 fig. and 6 pl., Feb. 1912. (*Incorporated in* Ill. Ent. Rep. 27, pp. 1-20, 1912.)

What should the state require of a negligent owner of a dangerous orchard? Jour. Econ. Ent., vol. 5, pp. 205-207, Apr. 1912. (*Also*, Ill. Hort. Soc. Trans., vol. 45, pp. 77-81, 1912.)

Report on nursery inspection and orchard insecticide operations. Ill. Hort. Soc. Trans., vol. 45, pp. 74-75, 1912.

The chinch-bug situation in Illinois: plans for a cooperative campaign. Ill. Agr. Exp. Sta. Circ. (*without serial number*), 7 pp., May 1912. (*Also,* Ill. Farm. Inst. Rep., vol. 17, pp. 302-307, 1912.)

The 1912 chinch-bug campaign in Illinois. Ill. Ent. Off. Circ., 17 pp., 8 fig., Nov. 1912.

The native animal resources of the state. Ill. Acad. Sci. Trans., vol. 5, pp. 37-48, 1912.

Twenty-seventh Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) sixteenth report of S. A. Forbes. 143 + xvii pp., 6 pl., 93 fig., 1912. Contents as follows:

What is the matter with the elms in Illinois? (1-20) [*issued as* Ill. Agr. Exp. Sta. Bul. 154, Feb. 1912]; on black-flies and buffalo-gnats (Simulium) as possible carriers of pellagra in Illinois (21-55) [*also in* Ill. Pellagra Com. Rep. for 1911, pp. 176-191, 1912, *and also issued separately,* 1912]; insects injurious to stored grains and their ground products, by A. A. Girault (56-82) [*issued as* Ill. Agr. Exp. Sta. Bul. 156, July 1912]; report on insects injurious to flowering and ornamental greenhouse plants in Illinois, by J. J. Davis (83-143).

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1909-1910. Ill. Univ. Rep., vol. 26, pp. 68-70, 1912.

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The corn root-aphis in Illinois. Ill. Agr. Exp. Sta. Circ. (*without serial number*), 7 pp., Jan. 1913.

The Simulium-pellagra problem in Illinois. Science, vol. 37, pp. 86-91, Jan. 17, 1913. (*Also,* Ent. Congr. (Oxford) Trans., vol. 2, pp. 477-485, 1913.)

[—with R. E. Richardson.] Studies on the biology of the upper Illinois River. Ill. Lab. Nat. Hist. Bul., vol. 9, pp. 481-574, with 21 pl., June 1913.

Biological and chemical conditions on the upper Illinois River. Ill. Water Sup. Assoc. Proc., vol. 5, pp. 161-170, with 1 map and 3 diagrams, 1913.

Needed legislation in the interests of horticulture. Ill. Hort. Soc. Trans., vol. 46, pp. 49-51, 1913.

The apple-flea-weevil in Illinois. Ill. Hort. Soc. Trans., vol. 46, pp. 53-55, 1913.

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1911-1912. Ill. Ent. Off. pamphlet, 24 pp., 1913. (*Cf.* Ill. Hort. Soc. Trans., vol. 45, pp. 74-81, 1912; vol. 46, pp. 49-51, 1913.)

Report of the committee on an ecological survey (of Illinois). Ill. Acad. Sci. Trans., vol. 6, pp. 18-23, 1913.

1914

The chinch-bug situation: present prospects and practical plans. Ill. Ent. Off. Circ., 3 pp., Feb. 1914.

The chinch-bug in Illinois in 1914: preparations for the summer campaign. Ill. Ent. Off. Circ., 11 pp., 3 pl., Apr. 1914.

Community operation and co-operation against fruit pests. Ill. Hort. Soc. Trans., vol. 47, pp. 171-176, 1914.

Fresh water fishes and their ecology. Ill. Lab. Nat. Hist., pamphlet, 19 pp., 10 pl., 21 maps, 1914.

Report of the Director of the State Laboratory of Natural History (to the Trustees of the University of Illinois), for 1911-1912. Ill. Univ. Rep., vol. 27, pp. 158-159, 1914.

[—with others.] Report of the committee on an ecological survey of the state. Ill. Acad. Sci. Trans., vol. 7, pp. 12-16, 1914.

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Recent Illinois work on the corn root-aphis and the control of its injuries. Ill. Agr. Exp. Sta. Bul. 178, pp. 405-466, with 18 fig., Jan. 1915. (*Incorporated in* Ill. Ent. Rep. 28, pp. 1-62, 1915.)

Observations and experiments on the San Jose scale. Ill. Agr. Exp. Sta. Bul. 180, pp. 545-561, with 3 fig., Mar. 1915. (*Incorporated in* Ill. Ent. Rep. 28, pp. 87-106, 1915.)

The chinch-bug in Illinois in 1915: preparations for the summer campaign. Ill. Agr. Exp. Sta. Circ. (*without serial number*), 16 pp., 1 map, 4 pl., Apr. 1915.

Preliminary note on the life history of the codling-moth in Illinois. Ill. Ent. Off. Circ., 15 pp., 6 pl., May 1915.

The danger to wheat from the Hessian fly. Ill. Agr. Exp. Sta. Circ. (*without serial number*), 1 p., Sept. 1915.

Twenty-eighth Report of the State Entomologist on the noxious and beneficial insects of the State of Illinois, (*being the*) seventeenth report of S. A. Forbes. 106 + ix pp., 24 fig., 4 pl. 1915. Contents as follows:

Recent Illinois work on the corn root-aphis and the control of its injuries (1-62) [*issued as* Ill. Agr. Exp. Sta. Bul. 178, Jan. 1915]; observations and experiments on the San Jose scale (63-79) [*issued as* Ill. Agr. Exp. Sta. Bul. 180, Mar. 1915]; life history and habits of the northern corn root-worm (80-86); the San Jose scale, by P. A. Glenn (87-106) [*issued as* Ill. Agr. Sta. Circ. 180, Apr. 1915].

The ecological foundations of applied entomology. Ann. Ent. Soc. Amer., vol. 8, pp. 1-19, 1915.

Insect and fungous pests of the nursery that should be classed as dangerous. Ill. Hort. Soc. Trans., vol. 48, pp. 153-161, 1 pl., 1915.

Insect pests and their control. Ill. Farm. Inst. Rep., vol. 20, pp. 187-191, 1915.

The insect, the farmer, the teacher, the citizen, and the state. Ill. Lab. Nat. Hist. pamphlet, 14 pp., 1915.

Executive report of the Illinois State Entomologist (to the Governor), including a report of operations under the horticultural inspection law, for 1913-1914; and report of the Director of the State Laboratory of Natural History. Ill. Ent. Off. pamphlet, 10 pp., 1915. (*Cf.* Ill. Hort. Soc. Trans., vol. 47, pp. 178-184, 1914; vol. 48, pp. 67-78, 1915. Ill. Univ. Rep., vol. 28, pp. 158-159, 1916.)

Report of the committee on an ecological survey of the state. Ill. Acad. Sci. Trans., vol. 8, pp. 15-17, 1915.

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A general survey of the May-beetles (Phyllophaga) of Illinois. Ill. Agr. Exp. Sta. Bul. 186, pp. 215-257, with 1 map, Feb. 1916. (*Incorporated in* Ill. Ent. Rep. 29, pp. 23-65, 1916.

The influence of trees and crops on injury by white-grubs. Ill. Agr. Exp. Sta. Bul. 187, pp. 261-265, Feb. 1916. (*Incorporated in* Ill. Ent. Rep. 29, pp. 66-70, 1916.)

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Robert Ridgway.

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OF

ROBERT RIDGWAY

1850-1929

BY

ALEXANDER WETMORE

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1931

ROBERT RIDGWAY

1850-1929

BY ALEXANDER WETMORE

Robert Ridgway, member of the National Academy of Science, for many years Curator of Birds in the United States National Museum, was born at Mount Carmel, Illinois, on July 2, 1850. His death came on March 25, 1929, at his home in Olney, Illinois.[1]

The ancestry of Robert Ridgway traces back to Richard Ridgway of Wallingford, Berkshire, England, who with his family came to America in January, 1679, as a member of William Penn's Colony, to locate at Burlington, New Jersey. In a short time he removed to Crewcorne, Falls Township, Bucks County, Pennsylvania, where he engaged in farming and cattle raising. David Ridgway, father of Robert, was born March 11, 1819, in Harrisburg, Pennsylvania. During his infancy his family removed for a time to Mansfield, Ohio, later, about 1840, settling near Mount Carmel, Illinois, then considered the rising city of the west through its prominence as a shipping center on the Wabash River.

Little is known of the maternal ancestry of Robert Ridgway except that his mother's family emigrated from New Jersey to Mansfield, Ohio, where Robert's mother, Henrietta James Reed, was born in 1833, and then removed in 1838 to Calhoun Prairie, Wabash County, Illinois. Here David Ridgway was married on August 30, 1849.

Robert Ridgway was the eldest of ten children. For a time his father's business in Mount Carmel prospered, but too generous extension of credit, a fire that destroyed his pharmacy, and a tornado in 1877 that partially demolished the rebuilt structure, brought financial reverses of such extent that, in 1877, he moved to a farm at Wheatland, Indiana. Later Robert Ridgway through savings from his employment in the Smithsonian In-

[1] An article by Harry Harris, entitled Robert Ridgway, with a Bibliography of his published writings, published in the Condor, Vol. 30, January, 1928, pp. 5-118, with numerous illustrations, has been of great assistance in the preparation of the present biography.

stitution purchased for his parents a more comfortable home at Olney, Illinois, where his father died on January 4, 1885, and his mother on December 14, 1886.

With parents interested in all living things outdoors, and a father with particular knowledge of birds for his period, it is not surprising that Robert Ridgway was early drawn to the studies in natural history that were to become his work in life. At this early day with the region only newly opened for settlement wild turkeys were found in numbers, the passenger pigeon, now extinct, abounded, and game of all kinds was abundant. Robert made frequent hunting trips with his father, armed with a muzzle loading shotgun made for him by an indulgent parent from a rusted rifle recovered from the sunken wreck of a river steamer, and as he grew older made many excursions alone. For ammunition the boy compounded his own gunpowder in his father's pharmacy. The art of taxidermy was unknown in this region, and Robert had only colored sketches as means to preserve for future examination representations of his birds. Aptitude in drawing was developed early, as his first sketches, still preserved, were made before he was four years of age. His drawing improved rapidly, and by the time he had reached his 'teens he had developed the discriminating eye for hue and form that marked his later, more practised work. His colors, like his gunpowder, he mixed himself with the resources of his father's pharmacy.

At this early period Ridgway supposed that no one else could be interested in birds. Through a few books—Goldsmith's "Animated Nature" with one volume devoted to birds, a History of the United States with a few pages on birds, and Goodrich's "The Animal Kingdom Illustrated" purchased for him at some sacrifice by his mother—he had learned of the work of Audubon, Wilson, Bonaparte and Nuttall, but believed that there were no living ornithologists.

Names for the birds that Ridgway observed and drew were a problem that was solved when the mother of one of his boyhood friends, Lucien Turner, later well known as a naturalist, suggested that he write to the Commissioner of Patents in Washington sending a drawing whose identity was a subject of discussion among the boys to see if the name of the bird depicted could

be ascertained. To this letter there came a reply from Spencer Fullerton Baird, then Assistant Secretary of the Smithsonian Institution, identifying the bird as the purple finch. This letter, dated June 23, 1864, was the beginning of Ridgway's contact with the Institution that he served so long. In subsequent correspondence Baird encouraged the young student to draw birds and mammals, to keep careful records of his observations, and to prepare skins and eggs. The young student showed such ready aptitude and progressed so rapidly that in the spring of 1867, when but seventeen years of age, through Baird he was appointed zoologist under Clarence King engaged in the Geological Survey of the Fortieth Parallel. After two weeks spent at the Smithsonian in Washington, Ridgway continued to New York where he joined the party that he was to accompany for the next two years. They proceeded by steamer to Panama, crossed the Isthmus, and then took boat for San Francisco arriving early in June. Field work began at Sacramento and continued east to Salt Lake City and the Uinta Mountains. In the fall of 1868 the party returned to Washington, and the summer of 1869 saw a continuation of this work in the Wasatch and Uinta Mountains.

On completion of this field experience Ridgway was engaged under Professor Baird to prepare the technical descriptions and some of the drawings for the great work on the birds of North America then under preparation jointly by Baird and Dr. Thomas M. Brewer of Boston. In 1874 he was assigned regularly to the Smithsonian Institution as ornithologist, and continued in that service until his death. Baird at this time becoming more and more engrossed in administrative details, assisted Ridgway in every way that he might to enable him to devote his attention uninterruptedly to the study of birds. With such efficient tutoring the development of the student was rapid so that immediately he was recognized as an authority on his subject. His work centered principally on American birds, and eventually led to the eight volumes on the Birds of North and Middle America published as Bulletin 50 of the U. S. National Museum between 1901 and 1919, with two more volumes in preparation at the time of the author's death. The technical information contained

in this work is exhaustive in its field and is basic for all similar studies of the present day.

In the early days of Ridgway's first work in Washington the Secretary of the Smithsonian Institution lived in the Smithsonian building, and a number of the workers, Ridgway among them, had living quarters assigned to them in various tower rooms. Here the subject of this biography was associated with other young naturalists who had connection with the Smithsonian, among whom were included W. H. Dall, Theodore N. Gill, Henry M. Bannister and José Zeledon. Others who worked more or less regularly on the collections at this time included Elliott Coues, H. C. Yarrow, C. Hart Merriam, Henry W. Henshaw and J. H. Kidder to mention only a few.

On October 12, 1875, Ridgway married Julia Evelyn Perkins, daughter of one of the engravers who had prepared the blocks for the illustrations for the History of North American Birds. To them was born one son, Audubon Wheelock, on May 15, 1877. This only child was interested like his father in the study of birds. Shortly after taking a position as assistant to Charles B. Cory, Curator of Zoology in the Field Museum in Chicago, Audubon Ridgway contracted pneumonia and died on February 22, 1901. From this shock Mrs. Ridgway never fully recovered and finally broke in health completely in 1921. Her death came in 1927.

Ridgway's first work in Washington was, as has been indicated, on an unofficial basis when he was employed by the publishers of the History of North American Birds of which he was joint author with Baird and Brewer. His official connection under the Smithsonian Institution began in 1874. In the annual report of the Smithsonian for that year he is listed as "Ornithologist." In 1875 his name is included as "In Charge of Ornithological Division." In 1877 he is indicated as "Assistant, Ornithology" and in 1880 as "Curator, Ornithology." The following year this is modified to "Curator, Department of Ornithology" and in 1883 to "Curator, Department of Birds." Personnel records of the National Museum indicate him as "Curator of Birds" in 1886, which title he held to the time of his death.

Mention has been made of the necessity for the manufacture of his own paints for his early drawings of birds from materials in his father's drug store. His later work in bird portraiture, with his written work in which he prepared detailed descriptions of birds in his various accounts of new forms, and other studies, brought definitely to his attention the need for a uniform series of named colors and the standardization of color names so that there might be no uncertainty as to the colors indicated in written terminology. This need found expression in his work on the Nomenclature of Colors for Naturalists and Compendium of Useful Knowledge for Ornithologists, published by Little, Brown and Co., in 1886. This work contained ten plates on which in small rectangles there were shown 186 named colors. The value of this contribution was at once recognized so that it became immediately a standard among naturalists, particularly among ornithologists, who with its aid were able to designate definitely for the first time the various shades that they used in their descriptions.

With this as a beginning Ridgway continued his studies of color on a more scientific basis, using in his experiments the color wheel and Maxwell disks. With the spectrum colors as a base he mixed two or three fixed colors in definite percentages by which he worked out a series of named colors that could be duplicated at any time. This procedure was of importance due to the fading of plates such as those produced in his first work on color and the subsequent change in value of the various color tones depicted. After prolonged effort extending through a period of many years he produced in 1912 a new and enlarged work entitled Color Standards and Color Nomenclature with 53 plates showing in small rectangles 1115 named colors. The plates were accompanied by tables giving the precise proportions of each mixture for each color. This work, published in an edition of 5000 copies by A. Hoen and Co., of Baltimore, Maryland, at once came into general use not only among naturalists but also among florists, manufacturers of paints, chemicals, wall papers and a variety of other industries. To insure absolute uniformity each color was produced at one time in sufficient quantity for the entire edition. This work is still in demand.

When Ridgway first came to Washington the scientific activities of the Smithsonian Institution were housed in the Smithsonian building, the only permanent structure in the Smithsonian park at that time. The bird collection when he first saw it in 1867 was on the first floor of the east wing, the living quarters of the Secretary being directly above. The smaller bird skins were kept in pasteboard boxes in tin herbarium cases, and the larger ones in glass-topped drawers in walnut cabinets. The offices were located in the section that connects the east wing with the main hall. Mounted specimens, more or less injured by water during the fire of 1865, were exhibited in the main hall.

About 1869 the collections were transferred to the middle room on the second floor of the North Tower adjacent to Professor Baird's office. Large specimens for which there was no room here were stored in cases and chests in the west basement. Ridgway had living quarters on the top floor of the south tower. About 1875 the former storage cabinets were replaced by what were known as "Salvin" boxes, with the skins in drawers. Some time after 1880 the present system of quarter-unit and half-unit cases provided with sliding trays was adopted, giving much greater facility in the handling of specimens. The smaller skins for a time were in the higher rooms of the South Tower, and then were transferred to the southwest gallery of the main hall, where they remained until brought into the Natural History Building in August, 1909.

Though with little taste for travel Ridgway made several trips for specimens for the Smithsonian. Three of these expeditions were to Florida. In February, 1895, he went with E. J. Brown and William Palmer to the Kissimmee region. In 1896 he was near Lake Okeechobee where he collected the Carolina paroquet. The following year he visited Fort Myer, and from there went up the Caloosahatchie River to Lake Flirt and Lake Trafford, and on into the Big Cypress where he obtained the ivory-billed woodpecker. When the Harriman expedition to Alaska was organized in 1899 Ridgway was invited to accompany the party and on this trip was absent from Washington from May 31 to July 30. Though he was successful in obtaining material for use in his work on North American birds he did

not enjoy the journey, confessing that he had little love for the North.

Two trips were made to Costa Rica, the first from December 8, 1904, to May 27, 1905, and the second from February 7 to May 8, 1908, both trips being planned to secure material lacking in the National Museum for his systematic studies of birds. Since a brief view in 1867 while en route through Panama to San Francisco it had been Ridgway's desire to study the life of a Tropical region. Through the assistance of Señor Don José C. Zeledón and Señor Don Anastasio Alfaro his work was rendered most effective, and excellent series of finely prepared specimens were obtained that were of great assistance to him in his writing.

The written contributions of Robert Ridgway to science are remarkable for the fact that there are among his early papers practically none of the amateur writings marking the enthusiasm of youthful observation before judgment of values has become mature and stable, that are found in the early years of most similar bibliographies. Under the wise counsel of Professor Baird, Ridgway on his return from the field for the Wheeler Survey embarked at once on the careful studies of birds and their relationship to one another, that continued throughout his entire life. His bibliography begins in 1869 with a few observations on the nesting of the belted kingfisher, and his second title, Notices of certain obscurely known species of American Birds, includes a critical review of certain thrushes and the grackles and description of some tanagers. In 1870 he published a classification of the North American hawks, a contribution that today contains much that is worthy of consideration despite the sixty years that have elapsed since its appearance. As has been noted on a previous page he was engaged at this time in preparing the technical descriptions for the History of North American Birds of which the three volumes on the land birds appeared in 1874. In 1877 there appeared his report on the ornithology of the Wheeler Survey, a work of more than three hundred quarto pages, in 1880 and 1881 his list of the birds of North America, and in 1884 the two volumes on the water birds of North America written in collaboration with Baird and Brewer for which Ridgway furnished the technical descriptions. His Manual of North

American Birds, a key to the identification of all known forms, appeared in 1887. The works mentioned have been those of major importance. Throughout this entire period there appeared constantly descriptions of new forms, comments on current ornithological questions, brief observations in botany, in herpetology, and in other branches of science, as well as longer papers dealing with detailed accounts of various collections that came to the Smithsonian Institution.

On the formation of the American Ornithologists' Union in 1883, Ridgway was naturally included among the twenty-three founders, and was elected one of the vice-presidents. He was also included in the membership of the committee entrusted with the task of preparation of an official check-list of North American birds. This volume appeared in 1886 after prolonged labor by the committee, in which Ridgway, due to his great familiarity with the subject, took a most effective part. The list when it appeared embodied what were then the modern ideas of the American school in the use of trinomial Latin names to designate geographic races, a logical procedure that aroused a storm of protest abroad where these forms were given the same binomial status as distinct species so that without special knowledge it was not practicable to distinguish between forms in these two categories. There was also included a code of nomenclature where definite rules of procedure for the adoption and use of scientific names were clearly and concisely set forth. In the preparation of the list in particular the clear and certain judgment and experience of Ridgway had a leading part. It may be remarked that the basic procedure outlined at this time has been continued to the present day, foreign workers in ornithology, having come gradually to the adoption of American methods as regards trinomials, while the code of nomenclature has been largely the basis for the preparation of an International Code covering scientific names in all branches of zoology.

Though Ridgway worked to some extent on birds of the Old World his efforts were centered principally on the avifauna of the Americas, particularly on North America, and Central America north of Panama. Preliminary work on a systematic catalog of the bird-life of this vast area began sometime after

1880, and notes were accumulated steadily until in 1894 Ridgway was directed by G. Brown Goode, then Assistant Secretary of the Smithsonian Institution, to make his principal official task the preparation of such a report. For this he was freed from administrative routine as much as possible, the burden of this being assumed by Dr. Charles W. Richmond who gradually relieved Ridgway until finally he had his entire time free for research and writing. In a task of this magnitude the ordinary official hours of labor from nine a. m. to four thirty p. m. meant little, and Ridgway worked early and late at a seemingly endless task. With removal of the bird collections to more commodious quarters in the Natural History Building he had at his command more spacious office and laboratory rooms, but with the growth of the Museum and increased interest in science everywhere he found the constant interruptions from friendly visitors more and more irksome in the delay that they occasioned in his task, so that he did much of his writing at home, coming to the Museum only to examine specimens. Attachment to the region where he had spent his childhood was an outstanding characteristic of the man, and in 1916 he removed to Olney, Illinois, where eventually he acquired two properties, the first a tract of eighteen acres located in the country, which he called Bird Haven, and which he developed as a bird sanctuary. Mrs. Ridgway's health was such that it was not advisable to live remote from town so that he obtained a home in the outskirts of Olney, which he called Larchmound, where he lived until his death, making regular visits however to Bird Haven and improving the grounds there as opportunity presented. In this seclusion he continued steadily at his task of writing, producing the eighth volume of his Birds of North and Middle America in 1919. With increasing age it became necessary for him to conserve his strength, and it was no longer possible for him to work the long hours each day that had been his custom. In the following years he frequently expressed a wish to retire, influenced in part by failing eyesight, and at one time made definite application for retirement but at my own urgent request was prevailed upon to continue. While there was realization that in all probability he would not see his

task completed it was highly desirable for the advance of science to obtain from him as much as possible of the results of his long years of study and observation. To this end he continued work on the last two volumes of the series, working particularly on the diagnoses of genera, families, and other higher groups.

As a worker of the older school Ridgway's writing was all in long hand, prepared carefully, with any corrections and interlineations required made with meticulous care. His manuscripts were completed in so legible and accurate a condition that they were given to the printer without necessity of being typewritten, being set in type directly from the long hand copy. To assist in his work thousands of specimens were measured for him by Mr. J. H. Riley and others, and Mr. Riley assisted also in compiling references for the synonymies given under each species. To Dr. Charles W. Richmond, Ridgway also owed much for careful and painstaking aid in the reading of proofs, a tedious task in which Dr. Richmond through long training has become particularly adept, aided especially through his comprehensive and exact knowledge of ornithological literature. The accuracy of the references in the completed volumes of Bulletin 50 owe much to this aid.

As recreation from his literary labors Ridgway chose horticulture and was particularly expert in his knowledge of trees and shrubs. Under his careful tending the grounds at Larchmound were landscaped with such skill and understanding that they attracted much attention, so much so that Ridgway was solicited by others for aid in developing estates. A number of papers dealing with the woody plants of his region were published.

Under such conditions Ridgway lived during the last years of his life. The death of Mrs. Ridgway in May 24, 1927, after fifty-two years of wedded life naturally affected Ridgway measurably. A widowed sister, Mrs. Lida R. Palmatier, came from California to take charge of Larchmound, and Ridgway continued his writing and his interest in the maintenance of his favored birds and shrubs until a few hours before his death on March 25, 1929, in his seventy-ninth year.

The rapidity with which Ridgway advanced from an unknown youth to one of the chief authorities on North American birds seems at this distant day almost incredible, particularly when it is recalled that he had no prolonged period of preliminary training but entered at once as a mere boy upon a definite program of constructive and serious research. His voluminous writings form the most important contribution to systematic ornithology as regards American birds of his day, as they are the basis of much of the work of his colleagues in the same field. As a young man he developed under the instruction of Baird the modern methods of research that prevail today, and was therefore, a pioneer in his field.

Friendly, but unassuming in personality, Ridgway made many friends, and was on the most cordial terms with his colleagues. Throughout this quiet popularity he had no desire for the public eye and avoided on every occasion anything that might draw to him undue attention. He attended few public meetings even of scientific organizations, and though he was elected to offices, particularly in the American Ornithologists' Union, did not participate in the public sessions. His active work for the Union came in the Committee on Classification and Nomenclature where, as has been said, his great experience was used in the building of the official check-list published in 1886, and in the second and third revisions of 1895 and 1910, respectively. His career as a whole gives definite instance of what may be accomplished by constant and persistent effort in one line of scientific endeavor. Generous and kind, he was particularly helpful to beginners, assisting them by word and act. It was natural from the leading place that he played in the ornithological bibliography of his period that Ridgway should be given membership in the leading ornithological societies of the world. In 1920 he was awarded the Daniel Giraud Elliot Medal of the National Academy of Sciences, and in 1921 received the William Brewster Medal. He was elected a member of the National Academy of Sciences in 1926. He was also the recipient of the Walker Grand Prize of the Boston Society of Natural History,

amounting to $1000, granted to him in 1913 particularly for his work on the birds of North and Middle America.

The name of Ridgway has been commemorated by his fellow workers in ornithology by two genera, twenty-three species, and ten sub-species of birds named in his honor. There is further to be mentioned the establishment of Bird Haven, as a permanent sanctuary for birds, a project that was much in the mind of Robert Ridgway during the last few years of his life. Originally a committee representing the American Ornithologists' Union, the Cooper Ornithological Club, and the Wilson Ornithological Club, undertook to raise an endowment, the intention being to secure a trust fund yielding sufficient income so that Ridgway might, if he desired, retire and spend his last years in peaceful quiet at Bird Haven. His death before this goal was reached changed somewhat the scheme. The Women's Clubs of Illinois were interested definitely in this plan which has been made possible finally by the interest of Mrs. Frances K. Hutchinson, who has acquired adjacent property to add to the Bird Haven holdings, and has arranged the necessary additional endowment to insure maintenance in perpetuity. The sanctuary will stand as a concrete memorial to Ridgway's name and to his interest in living birds.

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F. A. Gooch.

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OF

FRANK AUSTIN GOOCH

1852-1929

BY

RALPH G. VAN NAME

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1931

FRANK AUSTIN GOOCH

BY RALPH G. VAN NAME

Frank Austin Gooch was born in Watertown, Massachusetts, a suburb of Boston, on May 2, 1852. He died on August 12, 1929, in New Haven, Connecticut, the city in which he had made his home for forty-three years following a call to Yale as Professor of Chemistry in 1886.

Concerning his ancestry full information is available, largely from his own pen, since he was greatly interested in genealogy, and during the later years of his life compiled and printed an extended work on the Gooch family.[1] He was the elder child and only son of Joshua Goodale Gooch and Sarah Gates (Coolidge) Gooch. The father was a lumber merchant and holder of various town offices, and later, for twenty years, chief assessor of Cambridge, Mass. Both parents came of sturdy New England stock established in this country in the early days of the colonies, his father's line going back through John Gooch, born in England about 1600, who settled in Maine about 1640, and his mother's line through John Coolidge who came from England to Watertown before 1636. The lines of his father's mother (Goodale) and his mother's mother (Hastings) were of similar nature and origin, both established in Massachusetts by 1637. More will be said on a later page concerning the genealogical volume above mentioned, and the light which it throws on the more remote ancestry of the subject of this memoir. So far as his ancestry in New England is concerned, it is sufficient to say that it seems to have included a large proportion of men and women of more than average energy, integrity, and importance in the affairs of their respective communities. From such stock Frank Austin Gooch inherited a keen and active mind, a sound body, and the ideals of service which are reflected so clearly in his own life.

The story of his life, especially the earlier half, has been told by himself in an informal autobiography entitled "Trivial Reminiscences." This was written some five or six years before his

[1] "The History of a Surname, with some account of the Line of John Gooch in New England."

death, not for publication but, according to his own statement, chiefly for the edification of his two granddaughters, both of whom were at that time under fifteen years of age. It dwells chiefly on his boyhood, education, and travels, up to about 1884, while the portion dealing with his subsequent life, including his long and distinguished service as professor in Yale University, is disappointingly brief. The original typewritten copy was very kindly loaned to the writer by Dr. Gooch's daughter, Mrs. John D. Whiting, and has been the main source of information about his earlier life. A carbon copy, differing only in that it lacked the numerous photographs and pictures of persons and places which were bound with the original, was sent by Dr. Gooch, with other material, to the National Academy in 1926, in response to a request for biographical data. Charmingly written, this genial autobiography reflects clearly the kindly nature, modesty, and keen sense of humor of its author. Many passages from it will be quoted in the following pages, and where no source is mentioned, the use of quotation marks alone may be understood to indicate that the passage in question is taken from the Reminiscences.

The first fourteen years of his life were spent, except for summer outings at the seaside, in Watertown, then a suburb in the true sense, with all the attractions of woods, ponds, and open country near at hand, to arouse and foster an innate love of nature and of outdoor life. His formal schooling, begun in Watertown, was transferred when he was twelve years old to Mr. Atkinson's school in Cambridge, later known as the Kendall School, where he completed his preparation for college. Two years later his family moved to Cambridge "for easier accessibility to school, college, church, and the various social affiliations."

At the Atkinson school the curriculum included, in addition to the usual classics, history, and mathematics, a course in natural science illustrated by collections of stuffed birds and mounted insects, and by simple physical and chemical experiments. He writes in the Reminiscences:

"The experiments seen at school were of course incentives to imitation. My father indulged me in the possession of a few

chemicals and some simple apparatus. He personally helped me to make an electrical machine, using a large glass bottle for the revolving cylinder, and from a wide-mouthed preserve jar we made a Leyden jar. My earliest preparation of chlorine gas was made in the laundry, and there I was first 'gassed'—the precursor of many similar experiences.

"That this discursive curriculum was a good thing in my case and probably a determining factor in the subsequent choice of a career, I have no doubt; but I am sure that it interfered somewhat with the concentrated grind which sent boys from the Boston Latin School or from the Phillips Academy at Exeter with a better grammatical outfit for later work in the classical languages."

That his actual experiments in chemistry began some years before the preparation of chlorine above referred to, we learn from an earlier page of the Reminiscences.

"My uncle had dabbled a little in chemistry, as in most matters of intellectual interest, and had a small collection of simple apparatus and chemicals. From that stock my cousin Arthur and I got materials for the first chemical experiment in which I ever knowingly had a part. The day was dull and rainy and we decided that we might brighten things up a bit by making some gunpowder and starting a pyrotechnic display. The mixture of niter, sulphur, and charcoal was accomplished without accident and put in some sort of container inside a house built of billets of wood upon the kitchen floor. A train of powder was laid and touched off with a match while we retired to a distant corner to await the effects, which were perfectly satisfactory. My Aunt Lucy's arrival upon the scene of devastation and incipient conflagration prevented the repetition of my first chemical experimentation—more successful so far as the anticipated results were concerned than many since made—but candor compels the admission that Aunt Lucy's arrival was timely. When I was offered the opportunity some twenty years later to take charge of the manufacture of nitroglycerine at the Torpedo Station of the United States Navy, at Newport, the fondness for pyrotechnics had run its course in my case."

In 1868, at the age of sixteen, he entered Harvard College, and for the first two years followed, in accordance with the requirements, a course of study which consisted largely of the humanities. During junior and senior years, however, he took advantage of the greater freedom of choice permitted, to devote his time almost wholly to science, especially physics and chemistry, with the result that in 1872 he graduated with the degree of A. B. *cum laude,* and with *"summos in Physicis et Chemia honores."*

In the autumn of the same year, he began graduate studies at Harvard, principally in the fields of chemistry, physics and mineralogy. During the first year he served as a special instructor of a class in Chemical Philosophy, and in the next year he became an assistant to Professor Josiah Cooke, a position "which involved aiding in the preparation of Professor Cooke's brilliant and spectacular experimental Freshman lectures upon elementary chemistry and the holding of class-room recitations upon the subject matter of 'The New Chemistry'." In 1874-5 he served as assistant in the laboratory of quantitative analysis, and worked with Professor Cooke in his investigation of the atomic weight of antimony. This year also marked the beginning of his scientific publication, in the form of a paper entitled "On Two New Varieties of Vermiculites," on which his name appears with that of Professor Cooke as a joint author.

That the training which Gooch received from the association with Professor Cooke was of great value to him is certain. It also stimulated still further his interest in chemistry, and was undoubtedly an important factor in his subsequent choice of that science as the field for his life work.

He was also, however, greatly interested in the physics of crystals, and decided to spend the following year abroad in the study of this subject, either with Rosenbusch in Strassburg or with Tschermak and Schrauf in Vienna. In the summer of 1875, in the company of a classmate, Field, he sailed for Europe. The following is quoted, with some omissions, from the Reminiscences:

". . . We landed at Hamburg, . . . passed on to Berlin, where I met Jackson [2] (then an assistant professor at

[2] Loring W. Jackson.

Harvard on leave of absence) and made the acquaintance of Michael,[3] both of them studying with Hofmann, one of whose lectures I had the good fortune to hear. We three are now fellow members of the National Academy of Sciences. There also, I met under somewhat puzzling circumstances Herr Major Gerhardt (a brother of Mrs. Hagen, our neighbor in Cambridge), who held some sort of high position in the War Department. Mrs. Hagen had consigned to me for delivery to her brother a peculiar type of water-faucet in which she thought he would be interested, and she was to write to him of my coming to Berlin. By dint of persuasion I got a somewhat reluctant guard to take my card to the Herr Major and was admitted to his sanctum. The visit seemed to take him by surprise, although he was courteous; and our disadvantage was mutual in that he did not speak English and my knowledge of German was at least as deficient as was his of English. French did not help me much. His surprise seemed to deepen when I took leave of him without taking the device with me, and left no permanent address. Later, the matter was cleared up by a letter from Mrs. Hagen telling me that the arrival of her letter to her brother had been delayed until after my visit to him, and that while he knew I was an acquaintance of hers, he had been laboring under the impression that I was interested in the commercial introduction of a special device into Germany. However, I got away without being arrested as a spy upon the war department; and he got the faucet, which was after all the main object of my call.

* * * * *

"After a month in Dresden for practice in spoken German and another month of walking, mountain climbing and delightful travels in Switzerland, . . . Field and I parted at Strassburg. He went to Berlin, and I, after looking over the situation at Strassburg, decided to take up my work in Vienna."

His life in Vienna is described quite fully in the Reminiscences, for his stay there was made extremely interesting and pleasant by a friendship with Dr. Franz Steindachner, then a director of the Imperial Zoological Museum. Steindachner had

[3] Arthur Michael.

spent several years in Cambridge with Agassiz and having learned in advance, through a letter from a friend in that city, that Gooch was coming to Vienna, received him with the greatest cordiality. It was Steindachner who helped him to find comfortable lodgings, who accompanied him when he presented his letters of introduction to Tschermak and Schrauf, and secured for him the privilege of working on the collections in the Imperial Mineralogical Museum usually accorded only to workers of considerably greater experience, and in many other ways gave him most valuable assistance.

This was the beginning not only of a warm personal friendship with Steindachner, but also "of a long series of hospitable attentions from the entire Steindachner connection," which left Gooch in after years with the pleasantest possible memories of his stay in Vienna.

His work at the University of Vienna included, at first, attendance on the lectures of Tschermak and Schrauf, but as these lectures proved rather elementary after the training in mineralogy which he had had with Professor Cooke, they were soon dropped and the time devoted to laboratory work and research. At the beginning of the second semester he was planning to attend a more advanced course of lectures to be given by Tschermak, but was discouraged by that gentleman himself. At a loss to know the reason, Gooch finally learned from the assistant at the laboratory that the substance of this second course was Dana's System of Mineralogy, upon which he had been "brought up" at Harvard. His time was accordingly spent in studies upon the material at the petrographical institute, or at the museum, where a suitable microscope and a fine collection of rock sections were available, but except for occasional conferences with Tschermak he was left for the most part to books and his own initiative in prosecuting these studies. That his progress was rapid is shown by the fact that Tschermak intrusted him with the investigation of a set of sections of rock specimens from the Galapagos Islands, the results of which were published under Gooch's name in "Mineralogische Mittheilungen." This, and another brief paper in the same journal, his second and third scientific publications, both appeared in 1876. Outside of working hours much of his time

was spent in the congenial company of Steindachner. They were both fond of music and the theater, and frequently went together to concerts, operas, or plays, or visited the art galleries, or museums. Later, with the coming of spring, they made excursions to points of interest in the surrounding country, and once, even went as far as Budapest. Through Steindachner, Gooch was often invited to social gatherings of his relatives or friends, and through him again, had the opportunity to see certain ceremonies to which Steindachner's connection with the Imperial service opened the way. One of these was the formal court ceremonial, the "Fusswaschung," symbolic of humility, which is very graphically described in the Reminiscences.

In June, 1876, Gooch parted with regret from his warm friends in Vienna, and after short stops in Salzburg, Munich, and Paris, and a week in London, sailed for home.

Concerning the subsequent advantage to him of the studies pursued in Vienna, he writes as follows:

"That the work in petrography and upon the physics of crystals proved afterwards to have been but an interesting side issue, and of only occasional use, was determined by the trend of subsequent events. I have never regretted the time thus spent, and the opportunity to make the acquaintance of a charming coterie of associates at the museum—Tschermak, Brezina, von Reyer, Neminar—has left me many pleasant memories."

On resuming his graduate work at Harvard in the autumn, he devoted his time to a study of the action of zirconium tetrachloride upon sodium ethylate, and to work on the preparation of his thesis for the doctorate, which as presented later bore the title "A Treatise on Crystallography on the basis of Miller's System. With other papers." The "other papers" were his first three publications already mentioned, and a progress report on the zirconium work.

Meanwhile, he had been considering the plan of undertaking work in the field of thermochemistry with Julius Thomsen in Copenhagen, and early in June, 1877, having completed his work for the degrees of A. M. and Ph. D. (which at that time were coupled) and obtained permission to receive the two degrees *in*

absentia, he sailed again for Europe. Landing at London with time at his disposal, he took advantage of the opportunity for a three weeks' trip through Ireland, Wales, and England, in the pleasant company of Mr. Grundmann, Director of the Boston Art Museum, who had been a fellow passenger on the steamer. Their itinerary included, among many points of interest, visits to Manchester (Owens College), Oxford, and Cambridge. Concerning his visit to the last place, Gooch writes:

"At Cambridge, I sought out the residence of Professor Miller to whom Professor Cooke had given me a letter, but finding on inquiry at the door that he was ill and saw no visitors I left the letter and my card (without address) as a matter of courtesy. It was a complete surprise when an hour or so later a waiter at the hotel told me that a lady was waiting to see me and I found Mrs. Miller with an invitation to dine and the conditional promise that if it proved to be advisable I might have a few minutes' interview with her husband. So I dined with Mrs. Miller and her two charming daughters, and did get an unsatisfactory glimpse of an elderly, weak, and scholarly gentleman whose brief and concise 'Treatise' and 'Tract' upon Crystallography I had tried to simplify, as well as to extend, in my doctoral thesis. I am glad that I saw him, tho I doubt whether he really understood much more of me and my interest in him than that here was a stranger who desired to pay his respects to him. The kind and under the circumstances really unnecessary courtesy left me with a vivid appreciation of English hospitality."

After a few days in London, Gooch proceeded to Copenhagen. Here he was cordially received by Julius Thomsen, who showed him his apparatus, and some details of its manipulation, but explained that the apparatus was his own property (not that of the University), and that it would be necessary for Gooch to provide apparatus for his own use.

"He added, with a smile, that given the apparatus, he did not see why the work might not be done as well in Cambridge as in Copenhagen, and I was inclined to believe that he was at least half-right. With apparatus and the published papers at hand of Thomsen of Copenhagen and Berthelot of Paris there could

hardly be more difficulty in digging out a mastery of what had been done in their line than in acquiring facility in microscopic petrography with the aid of literature, a microscope, and a collection of Dünnschliffe, as I had had to do at Vienna. Moreover, as to further applications to chemical theory and really worth-while research along the lines opened up by Thomsen and Berthelot I felt skeptical. It looked to me as if the cream had been pretty effectually skimmed from the milk, and I think that the subsequent history of Thermochemistry has substantiated that view. At any rate, I made up my mind that the thing for me to do was to take advantage of the accessibility of the great laboratories, make notes upon the fittings, meet workers, and then get back to the work which I had left unfinished at Harvard. . . . So, the European venture resolved itself into a series of laboratory inspections which proved to have been of great advantage when I came later to make the fundamental plans of the Kent Chemical Laboratory of Yale College."

In accordance with this decision he planned the remainder of his travels in Europe to include stops at the seats of some ten or more universities, and in most cases, perhaps in all, paid visits to their laboratories. Among these were Upsala (Cleve's laboratory), Stockholm, Christiania, Bonn, Heidelberg (Bunsen's laboratory), Amsterdam, Antwerp, Edinburgh, and Glasgow, where he visited both the university and the important Tennant's Alkali Works. A few days also were spent in London, where Gooch had the great pleasure of meeting again (and for the last time) his good friend Steindachner of Vienna, and of visiting a number of points of interest in his company.

Gooch's return to Harvard was timely, for it resulted in his being offered, and accepting, an opportunity to work with Professor Wolcott Gibbs in his investigations of the Complex Inorganic Acids, taking his courses and doing the work of a private assistant. With the approval of the university authorities he also continued to hold during this year the Parker Fellowship to which he had been appointed early in 1876.

There can be no doubt that this association with Dr. Gibbs had the deciding influence in shaping Gooch's future career. He

seems up to this time to have had some leaning toward organic chemistry, but the training which he received under Dr. Gibbs developed in him a great interest in the problems of analytical chemistry, and remarkable ingenuity in devising apparatus and methods of value in analytical work. Here too, he must have acquired much of that skill and deftness in manipulation which enabled him to carry out difficult analyses with a certainty and accuracy which not many have equalled. From this time on, he turned definitely toward analytical chemistry as his chief field of work and of research.

The association with Dr. Gibbs lasted for two years, during the second of which he was "in fact as well as in function" Dr. Gibbs' private assistant. It was during this period that he published an extended paper "On the Estimation of Phosphoric Acid as Magnesic Pyrophosphate," which included a special study of the application of the method to the analysis of phosphotungstates and phosphomolybdates. Another and very important paper was one in which he described a new type of filtering crucible, now well-known under the name of "Gooch Crucible." The great value of this device lay in the fact that the filtering medium was a mat of asbestos fiber, introduced in suspension in water and compacted by suction. Not only does this supply an efficient filtering surface unattacked by most acids and readily adaptable to the needs of precipitates of different degrees of fineness, but precipitates which could not be heated in contact with paper fiber without undergoing decomposition could be collected, ignited, and weighed, in the filtering crucible itself, thus greatly simplifying the procedure. Criticised at first by some who attempted to use it with asbestos of inferior quality, but vindicated by the overwhelming testimony of others, this device soon became, and still is, one of the indispensable tools of the analytical chemist. The only reference in the Reminiscences to this important and useful invention reads as follows:

"It was during this period that I described in a paper 'On a new method for the separation and subsequent treatment of precipitates in chemical analysis,' a device which has proved to be of such general utility in analysis that, dubbed by the craft

with my surname (occasionally reduced to the level of a common noun), it has found its way into the 'Standard Dictionary' (of the English language), dragging me along with it."

His next position took him to Newport, Rhode Island. The nature of this work, and the circumstances under which he undertook it, are best given in his own words.

"In the autumn of 1879, at the invitation of Professor Raphael Pumpelly, who was then organizing a division for the census work, under the U. S. Geological Survey, with headquarters at Newport, R. I., I accepted an appointment as Expert Special Agent of the U. S. 10th Census, to undertake the microscopic examination of representative iron ores. The slicing and grinding apparatus was put in operation and the sections were made by an expert, but the opportunity never came to me to examine them in more than a cursory way (I believe that they are now in the National Museum at Washington). My activities were diverted to assist Blair[4] in making the chemical analyses which ultimately engaged the service of half-a-dozen experts simultaneously, and finally, when he resigned, the charge of that work (recorded in Vol. XV, (Pumpelly) U. S. Tenth Census) devolved upon me.

"In October, 1881, came the appointment as chief chemist of the Northern Transcontinental Survey which Mr. Pumpelly was organizing at the request of Mr. Villard to deal broadly with questions affecting the utilization and development of the natural resources of the region tributary to the Northern Pacific Railway System. This splendidly organized survey, which included in its personnel half-a-dozen men whom the National Academy of Sciences has honored with membership, came to an end in 1884 when the system was beset with financial difficulties and Mr. Villard's retirement brought about a change of policy. Contributions to Pumpelly's volume of the Census reports, one giving analyses of the lignities of the Northwest and another relating to the conversion of lignites into fuel of high heating power, the records of analyses of iron ores (twenty-one complete and one hundred-and-forty 'partial'), a little paper describing

[4] Andrew A. Blair.

the device of a tubulated crucible useful in determining volatile products of ignition, constitute the records published in my own name in connection with the activities of this survey."

In August, 1880, Gooch married Sarah Elizabeth Wyman, daughter of John Palmer Wyman, of West Cambridge, Mass., and for the next four years the young couple made their home in Newport, in pleasant surroundings and with a very congenial circle of friends.

During the winter of 1883, Gooch accompanied Pumpelly on a trip to Mexico. Mr. Pumpelly had been engaged by the owners of a mine, of which great things had been expected and on which a very large sum had been spent without much result, to inspect the property and to make a report. Gooch was taken along "to act as assistant in seeing that there should be no 'salting' of samples and to make such assays as might be necessary on the spot." Since the mine was situated in a very thinly settled part of Mexico, many miles from a railroad, the trip carried with it possibilities of adventure. On the railroad journey across Southern Arizona they passed near a region where Mr. Pumpelly twenty-two years before had spent eight months full of dangers and excitement in a stronghold of hostile Apaches, as manager of the "Santa Rita" mine. From Gooch's account of the trip we quote the following incident, which took place at this point:

"We were making the run from Benson to Nogales on the Mexican border when Mr. Pumpelly turned to speak and I leaned forward over the back of his seat to catch the remark. 'This is my old stamping ground. I never expected to get out of it alive.'—referring to his hectic experience of nearly a quarter-century before.[5] Then came a smash of glass, a whirr past my head, a sprinkling of powdered glass in the face—all marking the flight of a rifle-bullet through a window on the opposite side of the car across the car and out at the window just behind us on our side of the car. It was the shot of some drunken

[5] Described in Raphael Pumpelly's "Reminiscences," Vol. I, pp. 194-224.

cowboy or other irresponsible individual from out of the darkness at our isolated figures in the bright light of the car.

" 'That,' said Mr. Pumpelly, 'is the first call.' "

Leaving the railroad at Magdalena, they proceeded on horseback, with a guide, over more than a hundred miles of trails through rough country. At points of danger a sharp watch was kept for Apaches but none was seen,—"nor did we see any while we were in Mexico; although the Apaches did raid and killed men within nine miles of us at the mines, and arms were issued to the miners, and a watch set." That the situation was perhaps worse than is here implied, we gather from Mr. Pumpelly's account,[6] which states that the entire force of whites at the mines, some fifteen in number, was under arms for two weeks while the surrounding country was being raided.

The mine was duly inspected and the assays made. Rich ore was present but only in small quantities—nothing to justify the enormous sum which had been spent for machinery for a stamp mill many times larger than was needed,—and on the strength of Mr. Pumpelly's subsequent report the mine was closed.

Leaving Newport in the spring of 1884, Gooch went to Washington as chemist on the U. S. Geological Survey in charge of the chemical work of Yellowstone Park Division, which at that time consisted chiefly in analyses of the waters of the Park. This position he held for two years, spending the first field season in the Park and the remainder of the time in Washington, where the analyses were carried out. The results of numerous analyses of the Park waters, and the description of special methods used in making them, have been published, under the joint authorship of F. A. Gooch and J. E. Whitfield, in Bulletin 47, of the Geological Survey.

In 1886, an appointment to be Professor of Chemistry in Yale College, received the year before, became effective, and Gooch removed to New Haven to take up what proved to be his chief and permanent life work.

The first and principal task before Professor Gooch in taking up this position was to establish, for the benefit of candidates

[6] Raphael Pumpelly's "Reminiscences," Vol. II, pp. 653-4.

for the B. A. degree, a suitable group of elective courses in the fundamental branches of chemistry, inorganic, organic, and analytical. At the start all of the classroom teaching was done by himself, but other instructors were soon added, as the work grew.

Since his appointment included service in the Yale Graduate School, a second task, following naturally upon the first, was to provide courses and opportunities for research for the students who, attracted by his unusual qualities as a teacher and investigator, soon began to come to him for postgraduate studies leading to the higher degrees.

The first year was taken up with the planning and supervision of the building of the Kent Chemical Laboratory. His first class at Yale began work in the autumn of 1887, although the laboratory was not completed and ready for occupancy until the following spring.

Under Professor Gooch's wise and able direction, and furthered by the trend toward the study of chemistry on the part of students not definitely committed to a scientific career, the work of the laboratory grew steadily, and to such an extent that two additions to the building were made (in 1902 and 1906, respectively), which together more than doubled its capacity. By 1915 the number of students under instruction in the laboratory was well over four hundred.

It was the good fortune of the writer of these pages to begin the study of chemistry in Professor Gooch's undergraduate courses, to carry out his postgraduate research under his direction, and to return, two years later, for fourteen years of service (1904-1918) as a member of the teaching staff headed by him.

As a teacher of undergraduate students Gooch aimed always to lay a secure foundation for further study of chemistry. His lectures covered a great deal of ground, too much perhaps for the average student, but they were followed with great interest and profit by the better ones. In later years his lectures on inorganic chemistry had to be given twice to accommodate the large classes. Always clear and well prepared, they included a large number of lecture table experiments, set up in advance with scrupulous care, and thanks to his skill in manipulation these

experiments seldom failed to give a clean-cut and satisfactory result.

It was, however, in the more advanced work, and particularly in research, that he was most conspicuously successful in arousing in his students a love of the science and a real enthusiasm to contribute to its advancement. To research students needing his help he gave freely and abundantly of his time, spending many hours with each student, observing the experiments and often participating in their execution. His keenness in observation, and quick intuition in finding the cause of unexpected and puzzling results, were truly remarkable. He read extensively, and aided by an excellent memory had an extremely broad and comprehensive knowledge of chemical facts, theories, and procedure.

The field which interested him most, and in which the greater part of his research was done, was that of analytical chemistry, but he was far from desiring this field to be unduly emphasized in the work of the laboratory as a whole. On the contrary, he gave all possible encouragement and support to those students and members of his staff who were carrying on investigations in other fields, and he took the greatest satisfaction in the results which they achieved.

Throughout his period of service at Yale he was active in conducting research. At the time of his retirement, in 1918, more than three hundred research papers from the Kent Laboratory had been published, of which over eighty bore his name either as sole or as joint author. His actual contribution to the total was in reality much greater, for he often, actuated by generous motives, declined to have his name appear on papers the successful outcome of which had been largely due to his ideas and supervision of the work.

Professor Gooch's published work, though chiefly in the field of analytical chemistry, covered a wide range. He devised or perfected a large number of analytical processes and methods, some of which are now recognized as the standard ones and so described in current books of reference; others are less well known but have their utility in special cases.

His most important contributions dating from the period before his coming to Yale, are probably the Gooch filtering crucible, the quantitative separation of lithium from the other alkali metals by the action of amyl alcohol on the chlorides, and the estimation of boric acid by distillation with methyl alcohol and fixation by calcium oxide.

Among his later researches, carried out with various collaborators and published in the Kent Laboratory series, the following deserve special mention: Useful methods for estimating the elements molybdenum, vanadium, selenium and tellurium; two novel applications of sodium paratungstate in analysis,[7] which take advantage of the fact that it is non-hygroscopic and can be fused without change in weight; important studies of the conditions necessary for precipitating phosphates of the composition required for weighing as magnesium or manganese pyrophosphate; and, finally, a long series of methods for estimating various elements and radicals, based on the volumetric determination of iodine, a reaction which he favored and put to many uses on account of its exceptional advantages from the standpoint of convenience and accuracy. Indeed, so far as is known to the present writer, his contributions in the field of iodometric methods are unequalled in number and value by those of any other investigator.

Another important contribution was a method for the rapid electrolytic estimation of metals, using very vigorous stirring, a principle which effects an enormous saving of time, and is now in common use. Although the question of priority in this idea is somewhat involved, Gooch seems to have been the first to publish it (with H. C. Medway in April, 1903), and it is certain that he conceived it in complete ignorance of the contemporary work of others. The apparatus used by Gooch and Medway in this work is also noteworthy in that it dispenses entirely with platinum dishes and electrodes of special types, the metal being deposited on the exterior surface of an ordinary small platinum crucible, mounted by a rubber stopper on a vertical axle rotated

[7] As a better substitute for calcium oxide in the method of the estimation of boric acid, just mentioned; and in determining certain volatile acidic oxides by loss in weight on ignition.

by a small motor at a speed of six hundred revolutions per minute, or above. Since no large or specially shaped platinum parts are needed, the method is within the reach of even a very modestly equipped laboratory. Later, impelled by the growing scarcity of platinum, he devised a process for the rapid electrolytic estimation of metals, which employed electrodes of such small size that the total weight of platinum required for both anode and cathode was not over one gram.

The analytical methods devised by Gooch reflect as a rule his own practical turn of mind. As seen in the work on electroanalysis just described, he sought whenever possible to avoid elaborate or costly apparatus and time-consuming procedure, and often obtained the desired results by devices of exceptional simplicity. He was a firm believer in the need for more intensive and intelligent study of the problems of analytical chemistry as an essential factor in the progress of chemical science. He scouted the idea expressed by certain distinguished critics that analytical chemistry was a nearly exhausted field, and held that advances of immense practical value were both possible and certain, a view which the notable development of analytical chemistry during the last twenty years, especially through the application of physical methods, has already done much to justify.

Professor Gooch was the author, or joint author, of a number of books and pamphlets (listed in the bibliography at the close of this article), including a text on Inorganic Chemistry (1905), one on Qualitative Analysis (1906), and one on Quantitative Analysis (1916). Of these the "Outlines of Qualitative Analysis," in which his colleague, Philip E. Browning, was associated with him as joint author, is probably the best known. It was widely used, and five revised editions were subsequently printed, the last in 1928. In addition to the text on quantitative analysis mentioned above, he compiled in 1912 a book entitled "Methods in Quantitative Analysis originated or developed in the Kent Chemical Laboratory," and in 1901 he edited for the Yale Bicentennial Series a reprint in two volumes of the Kent Laboratory Research Papers, one hundred and eight in number, which had appeared up to that time. His book on genealogy,

referred to on an earlier page, was not formally published, but was printed for the author in 1926.

From the outset Professor Gooch had been in fact the director of the Kent Laboratory, although the actual title was not added until 1905, and the necessary administrative duties made constantly increasing demands upon his time and energy as the work of the laboratory expanded. Yet his great industry and systematic habits of work enabled him, with little or no assistance, to discharge the manifold duties of this position, and at the same time to give most conscientious attention to his teaching and his research.

As an administrator he was keen, far-sighted, and efficient. All routine matters were given careful thought and conscientiously attended to, and when a written report was called for, it was always ready on time. He would never allow unfinished work to accumulate, but when the demands upon his time were especially heavy, kept his desk clear by long hours of night work. As a consequence, he seemed always able to devote as much time as needful to his research students, and was never too busy to give a cordial welcome to those who wished to consult him.

His relations with his staff and more advanced students were most informal and friendly, and his wise counsel in many difficulties, both scientific and personal, was freely sought and generously given. Tolerant to a fault, he saw the good qualities in everyone, and when he erred, it was on the side of leniency in his judgments. His interest in those closely associated with him was sincere, and the respect and affection for himself which he inspired in his research students and the members of his staff, from professor to janitor, were truly remarkable.

Dr. Gooch was a rather tall man, erect and well built, a dignified and distinguished figure. He possessed a rare charm of manner, and his unusual courtesy and consideration for others were as sincere as they were unfailing. A man of real culture, with broad interests and a keen appreciation of the finer things of life, he was ready and interesting in conversation, and a most delightful companion. He was gifted with an exceptionally keen sense of humor, loved to hear a good story or to tell one, and his fund of entertaining anecdotes was well-nigh inexhaustible.

That such unusual social gifts should have been combined with a rather retiring disposition and a comparative indifference toward social activities seems strange, but the fact remains that he was not often seen at the gatherings of the various scientific societies of which he was a member, nor indeed at any gatherings except those connected with his college duties, or within the circle of his intimate friends.

He was to an unusual degree a lover of home, and of family life, and generally preferred to spend his leisure hours in the company of his wife and his daughter, their only child. It is noteworthy that in spite of the heavy burden of college duties he devoted a large amount of time to the education of his daughter in her earlier years, and took the greatest pleasure in doing so. He was fond of chess, and of music, of which he enjoyed a great deal at home, for his wife and daughter were both talented musicians. The bond of affection with his wife was an unusually strong one, and her death, in 1904, was a blow which cast a heavy shadow on all his subsequent life. He was fortunate, however, in retaining to the last the companionship of his daughter, who, after her marriage, continued with her husband to make her home with her father.

In June, 1918, he retired from active service at Yale with the title of Professor Emeritus. Though his activity in research had continued unabated up to this time, he never thereafter resumed experimental work, nor did he make any further contributions to chemistry except for collaborating with Professor Browning in two revisions of the "Outlines of Qualitative Analysis," in 1925 and 1928, respectively.

Instead, he turned with great interest and enthusiasm to genealogical research. His uncle, Samuel Henfield Gooch, had left on his death voluminous data and manuscript notes on the Gooch Lineage in New England. Starting with these, Professor Gooch gave much time and study for a number of years to the task of confirming and amplifying these data, and especially to the study of the Gooch line in Europe in pre-colonial times. Finally, in 1926, the results of his labors were printed in a book entitled "The History of a Surname, with some account of the Line of John Gooch in New England." This work gives evidence of

a very intelligent and comprehensive study of the available sources of information, both medieval and modern, and of keen and thoughtful analysis on the part of its author.

From it we learn that the surname Gooch is one of the phonetically similar forms, spelled in various ways, of the family name Goz or Guz, which appeared in Normandy in the tenth century. It appears to have been at first a descriptive name or cognomen, indicative of personal appearance or place of origin, applied to one of the companions of the invader Hrolf (progenitor in the male line of William the Conqueror) who came over from Norway in 912, but to have been adopted as a family name by the descendants of Hrolf Turstain, half-nephew of the famous Hrolf.

Following back the ancestry of a certain John Gooch of Alvingham, Lincolnshire, a younger son, whose identity with the John Gooch referred to on page 105 of this article as resident in Maine as early as 1640 is made very probable by a good deal of circumstantial evidence, Professor Gooch succeeded in proving that John Gooch was a direct descendant in the male line of Hrolf Turstain, a step covering twenty generations. Except for the slight uncertainty involved in the identification of John Gooch of Maine with John Gooch of Alvingham, Professor Gooch had thus traced back his own ancestry, in the direct male line, through twenty-eight generations, covering more than nine centuries, a success which would have been gratifying to any lover of genealogical research, and was undoubtedly so to him. His book contains also a great deal of information about related lines, as well as many references to the original sources.

During this period he took much satisfaction in giving his two granddaughters their early education, as he had done for his daughter a quarter of a century earlier, and it was at this time that he wrote for their entertainment his "Trivial Reminiscences," from which so many extracts have been quoted. No one who reads this interesting autobiography, which in typewritten form covers some seventy pages, can fail to be impressed both by the charm of the author's style, and by his unusual powers of memory, as shown by his detailed and vivid descriptions of the scenes and experiences of more than half a century before.

His former students and associates were always sure of a warm welcome at his home, and the regularity with which they took advantage of visits to New Haven to call upon their former teacher showed clearly the great affection in which they held him. And this feeling was mutual, for a friend more loyal and sincere than Professor Gooch would be hard to find. He took the deepest interest in the welfare and scientific careers of his former students, and enjoyed most keenly these opportunities to hear of their interests and problems, and to renew old friendships.

After a severe attack of pneumonia in 1925 his bodily health was never fully restored. Other periods of illness followed, and on August 12, 1929, he died at his home in New Haven, in the seventy-eighth year of his age, after an illness of several months duration.

So ended a life of unselfish devotion to duty, rich in useful work accomplished, and in the love and respect of many who had felt its guiding influence and the inspiration of its example.

Professor Gooch was a member of a number of scientific societies and academies. He became a member of the National Academy of Sciences in 1897. He was also an associate fellow of the American Academy of Sciences, and a member, or former member, of the American Philosophical Society, the American Association for the Advancement of Science, the Philosophical Society of Washington, the American Chemical Society, the Connecticut Academy of Arts and Sciences, the New York Academy of Sciences (corresponding member), Sigma Xi (Yale Chapter), and of various other organizations of a scientific or social character, including a number of clubs.

Two brief biographical articles on Professor Gooch have already been published, both from the pen of Professor Philip E. Browning; one, in the series "American Contemporaries," in *Industrial and Engineering Chemistry*, *15*, p. 1088, (1923); the other, an obituary, in the *American Journal of Science*, 5th ser., *18*, pp. 539-540, (1929).

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1892

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1893

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1895

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1901

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1905

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1912

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1880

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Frank Wigglamuth Clarke

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FRANK WIGGLESWORTH CLARKE

1847-1931

BY

L. M. DENNIS

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FRANK WIGGLESWORTH CLARKE

March 19, 1847, to May 23, 1931

BY L. M. DENNIS

Some two centuries ago, Robert Clarke, a member of the Scotch colony near Londonderry, Ireland, came to this country and settled in Londonderry, New Hampshire, about 1725. A grandson of Robert, Samuel Clarke, was a Unitarian Minister located first at Princeton, and later at Uxbridge, Massachusetts. A son of Samuel Clarke, Henry W. Clarke, married Abby Fisher, and their son, Frank Wigglesworth Clarke, the subject of this memoir, was born in Boston on March 19, 1847. Professor Clarke's grandmother on his father's side was Sarah Wigglesworth, granddaughter of Col. Edward Wigglesworth of Washington's army, and great-great-granddaughter of Michael Wigglesworth, the author of the "Day of Doom," a Puritanical poem that was popular in the 18th century. Michael was descended on the mother's side from Anthony Fisher, who settled in Dedham, Massachusetts, about the middle of the 17th century.

Professor Clarke's father, Henry, was first a hardware merchant in Boston, and later a dealer in iron-working machinery. His mother died when he was about ten days old, and Clarke's earliest childhood was spent with his grandfather at Uxbridge. In 1851 his father married again and lived for about eight years in Woburn, ten miles from Boston. He later removed to Worcester, but returned to Boston in 1866, making it his home in the suburb of Watertown, where he resided until his death in 1907.

From 1857 to 1869 Frank's time was mainly divided between his father's home and that of his grandmother Fisher in Boston. As a result of this frequent moving about, his early education was disconnected. He began his regular schooling in Woburn, was then for sometime at Uxbridge, and following a year or two in a boarding school at Stoughton, Massachusetts, he attended several different schools in Boston. After a year in the Boston Latin School he went to the English High School, and in March 1865 he entered the Lawrence Scientific School of Har-

vard University, where he took up the study of Chemistry under Wolcott Gibbs, receiving the degree of B. S. in 1867.

But as Professor Clarke was wont to say, "formal schooling is only a small part of education." He learned to read before he went to school, and he thought that he picked up his letters from blocks that had been given to him for playthings. Even before entering his first school at Woburn he was reading Robinson Crusoe and the Arabian Nights. He recalled that at Woburn he was particularly interested in Colburn's mental arithmetic, from which he derived valuable training. Problems were given out which had to be clearly stated by the student and then orally solved in a logical manner. Clear reasoning was required, and he felt that that training stood him in good stead throughout his life. He was early interested in geography, but he greatly disliked the study of grammar, although he was a very accurate speller, an accomplishment that he thinks was developed by his omnivorous reading, in which he was later given opportunity to indulge through access to the public library in Boston.

Like all healthy boys he greatly enjoyed outdoor sports, such as swimming, boating, and fishing, and he early developed the collecting instinct, being interested in coins, pressed flowers, and especially in minerals. His interest in minerals seems to have dated from the time when a visitor at his grandfather's gave him a pebble of chlorastrolite. This was the beginning of a small mineral collection and his interest in mineralogy. As a youth he visited the well-known mineral deposits at Lancaster and Boulton, Massachusetts, and on this and other excursions he learned much concerning the natural occurrences of minerals.

His interest in chemistry seems to have sprung from the perusal of a copy of "The Boys' Own Book" which was given to him when he was about eleven years old. The section on chemical experiments fascinated him, and a few years later, in association with some of his playmates, he got together a small supply of cheap apparatus and chemicals with which they had, as he expressed it, some successes and many failures. But in this boyish study he acquired some knowledge of manipulation of apparatus which was of value to him when he later entered the Lawrence Scientific School.

Allied to his interest in making collections of specimens was his bent for arranging his information in tabulated form. When he was about eight years old he was given a copy of Mayne Reid's "Boy Hunters" in which many birds and plants were described. Fascinated by their names, he carefully tabulated their characteristics without any suggestions from older people. This instinctive urge to tabulate data led in later days to the production of some of his most important and valuable scientific work.

After graduation from the Lawrence Scientific School in 1867, he remained there for an additional year to carry on research, and in March 1868 he published his first scientific paper, "A New Process in Mineral Analysis." During that year he also lectured on chemistry in the newly established Boston Dental College. In January 1869 Clarke went to Cornell University as assistant to Professor J. N. Crafts and remained there until the close of the academic year. While at Cornell he found much pleasure in exploring the many ravines near Ithaca and wrote a guide book to that region which was published by a local concern. He also there worked out an excellent method for separating tin from antimony.

The next four years were spent in a rather itinerant manner—lectures in the Boston Dental College, in a private school, and in some popular courses, reporting Tyndall's Lowell Institute Lectures for the *Boston Advertiser,* journalistic work, articles to several magazines, and occasional verses—anything to add to his small income.

He did not, however, neglect his scientific work. In 1873 he wrote for the *Popular Science Monthly* an article entitled "Evolution and the Spectroscope," pointing out that the evolution of planets from nebulae had been accompanied by the evolution of the chemical elements from their simplest forms. A "Table of Specific Gravities, Boiling Points, and Melting Points of Solids and Liquids" was accepted for publication by Joseph Henry, Secretary of the Smithsonian Institution, who made it the first of a series of articles to which he gave the name "Constants of Nature." This was later followed by other similar tables, several of them by Clarke, and one by G. F. Becker. In 1873 he

received appointment, largely through the influence of Professor Henry, to the Professorship of Chemistry, Physics, etc., at Howard University in Washington. In the following year he married Mary P. Olmsted, of Cambridge, Massachusetts, and in September he went to the University of Cincinnati as Professor of Chemistry and Physics, where he remained for nine years.

At Cincinnati his duties at first were largely routine, but he soon equipped a small but satisfactory laboratory and published several investigations. It was while there that he wrote his noteworthy article on the "Recalculation of the Atomic Weights" which appeared as one of the series of the "Constants of Nature" of the Smithsonian Institution.

In 1883 Clarke was appointed Chief Chemist to the United States Geological Survey at Washington, and Honorary Curator of Minerals of the United States National Museum, and these positions he held until his retirement on December 31, 1924.

The work in the Survey was of the most varied character, comprising many routine analyses to assist geologists, analyses of waters of the Yellowstone Park, and of igneous rocks and rare minerals. Personally he carried out investigations in the field of Geochemistry, and he always regarded his researches in this field as his most important scientific contributions. The results were summarized in his volume "The Data of Geochemistry," which ran through five editions issued as Survey Bulletins.

Apart from his regular duties as Chemist for the Survey, Clarke was called upon for service in connection with the government exhibitions at several expositions, and at the close of the Paris Exposition in 1900 the French Government awarded him the decoration of chevalier of the *Legion d'Honneur*.

In 1903 the centenary of Dalton's Atomic Theory was celebrated at Manchester, England, and on that occasion Clarke delivered the Wilde lecture and received the Wilde medal. In 1909 he delivered before the Chemical Society of London a lecture in memory of Wolcott Gibbs, his former teacher. Many universities conferred upon him honorary degrees and he was associated, either as member or as honorary member, with many scientific societies in this country and in Europe.

Not long after the appearance of the first edition of Dr. Clarke's "Recalculation of the Atomic Weights," three similar but less complete revisions were published in Europe, by Meyer and Seubert and by Ostwald in Germany, and by Sebelin in Norway. The American Chemical Society, recognizing the fundamental importance, both to scientific research and to the chemical industries, of correct data concerning the atomic weights of the elements, appointed Dr. Clarke a committee of one to prepare an annual report on the subject, and annually to compile a table of atomic weights for official use in this country. His first report was presented to the Society at the Baltimore meeting in December, 1893. In 1897 the need for an official table of atomic weights for German analysts was brought before the German Chemical Society, and a committee of three, Landolt, Ostwald and Seubert, was appointed to consider and report upon the question, which they did on October 31, 1898. They used Oxygen = 16 as the basis for the atomic weights, a value that Clarke had provisionally adopted for his table of 1893. In their report Landolt called attention to the desirability of international agreement concerning these values, and the German Committee issued on March 30, 1899 an invitation to the Chemical Societies and similar organizations of the United States and the leading countries of Europe to nominate members of an International Committee. The Committee thus created consisted of more than fifty members, the representatives of the American Chemical Society being J. W. Mallett, E. W. Morley, T. W. Richards, E. F. Smith, and F. W. Clarke, Chairman.

It was soon found that this large committee was too unwieldy for efficiency, for its work had to be carried on by correspondence which caused long delay in the preparation of its report. For this reason the members of the Committee from the German Chemical Society addressed to the members of the large Committee the question as to whether the periodic preparation of a table of Atomic Weights should be placed in the hands of a much smaller committee. The answers to this inquiry being strongly in the affirmative, the members were asked to send in nominations for a committee of three. Those receiving the highest number of votes were F. W. Clarke, America, T. E.

Thorpe, England, and K. Seubert, Germany. Clarke was asked to act as chairman of the committee, a position that he held until 1918 when the World War caused the temporary discontinuance of the International Committee.

One of Dr. Clarke's greatest services to chemistry in this country was in connection with the creation of the American Chemical Society. Up to 1873, chemistry had been given but scant attention in the meetings of the American Association for the Advancement of Science. In that year, at the meeting at Portland, Maine, four young men, C. E. Munroe, W. McMurtrie, H. W. Wiley and F. W. Clarke, presented a request that chemistry be more adequately recognized by the formation of a subsection of chemistry, now Section C. The request was granted, and the section held a successful meeting at Hartford in the following year. In 1876 the chemists of New York City organized a local society to which they gave the name American Chemical Society, and some eight years later another local society of chemists was formed in Washington. The American Association met at Cleveland in 1888, the chairman of the chemical section at that time being Dr. C. E. Munroe. Dr. Clarke wrote to him and suggested the formation of a really national Chemical Society. Dr. Munroe favored the idea, and after some three years of discussion of the project, in which Dr. Clarke took an active part, a compromise proposal of the New York Society was adopted. This was, that if the chemists of the country would accept the name and charter of the New York organization, that society would form a local section of a truly national society, the Washington society to take the same action. This national society has now become the largest chemical society in the world, with eighty sections and nearly 19,000 members. Dr. Clarke was elected to the presidency of the society in 1901.

The numerous articles from his pen upon a wide variety of subjects that appeared in divers journals and magazines furnish abundant evidence of the breadth of his interest and the scope of his knowledge and of his gift of felicitous and convincing expression.

He was one of the most kindly and lovable of men, simple in his tastes and modest and unassuming. His sense of humor and ready wit gave to his conversation a delightfully piquant flavor and it was a most entertaining experience, which the writer often had the privilege to enjoy, to listen to his reminiscences, sometimes keenly critical, sometimes highly amusing, but always sympathetically appreciative, of the noted men whom he had known and of their scientific work. His was a long life, a happy and useful life, a life of helpfulness to others and of high achievement.

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FRANK WIGGLESWORTH CLARKE

COMPILED BY LUCIA K. WILLIAMS

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OF

EDWARD CHARLES PICKERING

1846-1919

BY

SOLON I. BAILEY

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1932

EDWARD CHARLES PICKERING

BY SOLON I. BAILEY[1]

Edward Charles Pickering was born on Beacon Hill, Boston, July 19, 1846. He died at Cambridge on February 3, 1919. At the time of his death the Observatory was less than eighty years old, and he had been Director forty-two years, a period considerably longer than the combined terms of his three predecessors.

Mr. Pickering was fortunate in his heritage. Of a family always prominent in New England history, he was heir neither to riches nor to poverty, but to splendid opportunity, which he eagerly grasped. From early youth to old age, his zeal in the pursuit of scientific problems was unbounded. His education was begun in private schools, but later carried forward at the Boston Latin School. He had small love of the classics and gave them scant attention. In the Lawrence Scientific School, however, he entered upon his work with that enthusiasm which marked all the activities of his mature life. He was graduated from this school *summa cum laude* at the age of nineteen, and was immediately appointed Instructor of Mathematics in that institution. A year later he became Assistant in Physics at the Massachusetts Institute of Technology, and in the following year Thayer Professor of Physics, a position which he held until he became Director of the Observatory. During his ten years at the Institute, the history of his work is the history of the Department of Physics. His appointment as Thayer Professor came at the urgent request of President William B. Rogers, the former occupant of the chair, who wrote to Pickering regarding it:

"Let me say that, with all the urgency of other Institute duties, I should be quite unwilling to relinquish it to any other successor, so much do I love its exercises, and so sure am I that under

[1] This biography has been taken from Professor Bailey's "History and Work of the Harvard Observatory," with only slight alterations. The bibliography has been prepared by Jenka Mohr.—Harlow Shapley.

your direction they will preserve the breadth and character which it has been my aim to give them."

At this time Pickering was only twenty-two years of age. During the busy years of his professorship at the Institute, forty-one scientific papers were published by him, or by students under his direction, as well as two volumes of his pioneer textbook entitled "Physical Manipulations." He established in connection with these volumes the first physical laboratory in America for students. The idea of such a laboratory had been suggested by President Rogers, but its successful installation and management were due to Pickering. The importance of this development was widely recognized; it has been regarded as marking an epoch in the teaching of physics. Pickering measured the value of the course in physical manipulation by its success in teaching the student to think for himself, and in fitting him to solve problems experimentally. Research was Pickering's chief interest, although teaching consumed the greater part of his time. His own investigations, on the subject of light, formed a fitting foundation for his future life work.

In 1869 and 1870, Mr. Pickering took part in the expeditions sent out by the United States Government to observe the total solar eclipses. He introduced at the Institute a course of lectures for older students on geodesy and topography, and one on practical astronomy, especially for engineers. He also designed a spectrometer, which was constructed by Alvan Clark and Sons, and was the most powerful instrument of its kind at that time.

A notable contribution of a different character was made by his early experiments with the telephone. In 1870 he constructed a receiver consisting of a flexible iron diaphragm supported at the edges and replacing the armature of an electromagnet. The apparatus appears to differ in no way in principle from the receiver later in use. He would not consider protecting the device by patent, since such a course would have been contrary to his code of ethics.

In 1876 Pickering founded and became first president of the Appalachian Mountain Club. The great value of this club for popular purposes is well known, but his primary aim was the

furthering of health and science. He perfected a portable 12-pound micrometer level for the rapid determination of approximate positions and altitudes. With this instrument he made thousands of observations of various points of interest in the White Mountains. The intensity of his interest and the enthusiasm and success with which he carried out his plans made a deep impression on his associates.

Professor Pickering was chosen Director of the Harvard Observatory in 1876, and entered upon his duties on February 1, 1877. The appointment of a physicist to direct an astronomical observatory caused some criticism from astronomers of the old school. There was no lack of candidates for the position among astronomers of experience and reputation. President Eliot, however, who called him to the directorship of the Observatory, was thoroughly familiar with the unusual scientific and administrative ability which Pickering had shown at the Massachusetts Institute of Technology.

Pickering found the times propitious for the introduction of new methods. The old astronomy of position and motion which had occupied the chief place in the programs of the great observatories in the past was destined soon to be pushed into the background by the urgency of astrophysical problems. Even the determination of magnitudes of stars had not been placed on a sound scientific basis and comparatively little was known as to their nature. Everywhere there was a great dearth of facts. In such a condition of the science, Pickering decided that the accumulation of great masses of data would constitute the greatest contribution he could make to the advancement of astronomy. Theories in regard to the structure of the stellar universe could wisely be deferred until better foundations were provided.

Pickering's first care at the beginning of his administration was to secure additional funds for carrying on observations, and for publishing those already made, and also for the extension of his investigations into new fields. His first Annual Report contained an appeal for financial aid, and every succeeding report included some such direct or indirect appeal. The great schemes he was planning could be carried out only through the assistance

of many minds and many hands, and these could be obtained only by a great increase of endowment. Little by little this was secured. His own part in this increase was considerable. In all, he gave to the Observatory more than a hundred thousand dollars.

The first and one of the greatest of Pickering's achievements was in stellar photometry, which for many years was his leading interest. He found the Observatory equipped with two instruments of the highest class for that time: the large 15-inch equatorial refractor, and the new 8-inch meridian circle obtained by Winlock. For a while he carried on investigations with photometers attached to the large refractor, but later his observations became more and more photographic. These early labors held particular interest because of the measurement, in 1877, of the newly discovered satellites of Mars. While he was engaged in carrying on these investigations, a meridian photometer was constructed for the convenient measurement of the magnitudes of all the brighter stars.

Considerable work bearing on the structure of the Milky Way was done by Pickering during his long directorship. In connection with his photometric catalogues, he discussed the distribution of stars of different magnitudes. Dividing the stars into groups half a magnitude apart, he studied the distribution of the 4193 stars of the early Harvard Photometry, together with that of the 324,000 stars of the Northern Durchmusterung, and the 7363 stars of the Uranometria Argentina. Pickering found the actual number of stars observed was less than that indicated by theoretical discussions, on the improbable but convenient assumption that the stars are of equal brightness and uniformly distributed in space.

The improvement of the photographic dry plate came at the beginning of Pickering's administration, and its possibilities were promptly grasped by him. Something of romance was perhaps lost by the introduction of photographic methods, but the gain in efficiency was tremendous. Charting a field of stars, formerly a labor of weeks or months, could be accomplished in an hour, the resulting photograph often showing more stars

than could be seen by the eye with a telescope of equal size. The creation of the library of celestial photographs, mainly of stars, containing more than 200,000 original negatives, was an unique achievement, involving the foundation of the most valuable and irreplaceable astronomical collection in the world, and destined in time to give a history of the sky.

Pickering early saw the possibility of photographic photometry and made many experiments and observations. For many years the difficulties were too great for its successful use, but before the close of his life these had been overcome in large part. He conceived the idea that a large collection of celestial photographs, covering the whole sky and repeated at short intervals over a long series of years, would have immense value, and he attempted to make this record of the stars as complete as possible. An auxiliary station was founded in the southern hemisphere in order to cover the southern sky. Photographs of various kinds were taken; the spectra of the stars were obtained with the objective prism. Records were made with instruments of widely different powers: at one extreme the 24-inch Bruce doublet, which, with an exposure of one hour, showed stars to about the seventeenth magnitude; and at the other extreme a wide-angled one-half-inch Ross-Zeiss lens covered a field about 60 degrees square, so that the entire sky available could be covered in a single night with exposures of one hour, stars to about the ninth magnitude being photographed.

These half-examined plates, made, in many cases, only for the purpose of securing as complete a record as possible, appeared to many as unnecessary and extravagant, and even excited ridicule. This attitude seems absurd now that their value has been so fully demonstrated. Hardly a new star or variable has been discovered in recent years whose history could not be traced in a large degree upon these photographs. The extensive discoveries of novae, asteroids, variable stars, and other interesting celestial objects from this collection of photographs are ample proof of its value. A series of plates having exposures of four hours with the 24-inch Bruce was proposed, and a considerable number of excellent photographs were made at

Arequipa from the South Pole northward. Such a series, if it could be completed for the whole sky, would contain a hundred million or more stars, and from it might be derived definite lists of clusters and nebulae for the determination of their distribution, motions, and distances. The scheme, however, would require a long time for its completion with a single telescope, and meanwhile the Selected Areas of Kapteyn, Pickering's own Standard Regions, and other cooperative plans made this complete plan less necessary.

With the development of photographic methods, Pickering included some work on the solar spectrum in the general study of spectral classification, paying special attention to the varying intensity of the atmospheric lines in the solar spectrum as affected by variations in temperature, moisture, and other meteorological conditions. He also did pioneer work on line intensity for the spectral regions around the Fraunhofer line E, and thus helped to pave the way for the future investigations of solar and stellar atmospheres.

The study of stellar spectra, carried on by several observers under Pickering's direction, constitutes one of the greatest achievements of the Observatory. Toward the close of his life, the completion of the Henry Draper Catalogue of stellar spectra, for which the classification of more than two hundred thousand stars was done by Miss Cannon, absorbed Pickering's attention. This catalogue, consisting of nine volumes of the Annals, was nearly finished at the time of his death. To estimate its importance, one needs only to remember how small was our knowledge of the nature of the stars in 1885, when he began to photograph them with the objective prism, and to consider how intimately the Harvard classification has entered into nearly all lines of astronomical research.

Aside from the classification of spectra, the objective prism plates yielded enough in the way of by-products to justify Pickering's enthusiasm: several novae, hundreds of new variable stars, and long lists of peculiar stars of special interest. Nothing pleased him more than to know that the results obtained at the Observatory were those most needed by astronomers in

their investigations. Certainly no better example could be found of a recognized and fulfilled astronomical need than the classification of stellar spectra in the nine volumes of the Henry Draper Catalogue, as carried out by Miss Cannon.

It is possible that Pickering's best work was in photometry and spectroscopy, but he was active in many other fields. The study of variable stars was a marked feature of the Observatory work during his administration. When he began his observations, about 200 variables were known; at the time of his death 3435 variables had been found at the Harvard Observatory. He published, in 1880, a classification of variable stars which is the accepted notation at the present time. He soon began to encourage their observation on a scale hitherto unknown. This was possible not only through the increasing resources of the Observatory, but also through the assistance of amateurs. When the American Association of Variable Star Observers was formed, he gave the members the assistance which they needed. The spirit in which this was given and received is well shown by the regard and affection in which he was held by the members of the Association. At their meeting in 1918 they presented him with a beautiful gift, after their president had made the following reference to him: "He has assisted us in everything that we have undertaken, and has carefully watched our progress along every step of the way, and the manner of his so doing has been that of the Big Brother."

The astronomy of position was not neglected during Pickering's directorship, although his chief interests lay in astrophysical lines. Two zones of the Astronomische Gesellschaft, those from $+49° 55'$ to $+55° 10'$, and from $-9° 50'$ to $-14° 10'$ declination, were observed and published during that time, although the observations for the former zone were begun under Professor Winlock. Altogether, the work of the meridian circle occupied the time of one professor and several assistants during half a century, the results filling a dozen volumes of the Annals.

When Pickering came to the Observatory, only a dozen volumes of the Annals had been published or were ready for

printing. At the time of his death, about eighty of these quarto volumes had been issued or were practically ready for the printer. Many of these, indeed, were chiefly the work of others, supervised or edited by him. On the other hand, an enormous amount was his own. He was a natural leader, but he was an indefatigable worker as well. He worked for the real love of it, carrying on observations for several hours each clear night, in addition to his arduous duties as director. Of the two million observations concerned in the visual Harvard Photometry, more than half were made by him.

Pickering's interest in the work of others seemed as intense as that in his own. His desire was to secure the largest possible results. If he was fond of quantity, the care with which he examined and reexamined all that he did is evidence that quantity was not sought at the expense of quality. Loyalty to his predecessors in office was one of his marked characteristics. He devoted much time and badly needed financial resources, during the early years of his directorship, toward completing and publishing their unfinished work.

As unusual as were Pickering's scientific accomplishments, his personal qualifications were equally rare. For men and women he had an equal charm. His grace of manner and conversation captivated all those who knew him intimately. To all who seemed to have any claim upon him, he gave a courteous regard. He seemed always able to draw out a person's best qualities, and to leave him with the rare and happy sense of having found at last real appreciation. To astronomers especially he was ready with unlimited service, and he is remembered by many as an ideal host.

Pickering thoroughly believed in the advantage of broad associations for the good of science and mankind. One of the most cherished objects of his life was to secure an international fund for the benefit of astronomers of all nations. Of a similar nature was his plan for an international southern telescope which would be devoted to the needs of astronomers anywhere.

Believing that the best service he could render to astronomy was the accumulation of facts, to this end he massed all the

forces he could command, instituting great pieces of research, sometimes employing many routine workers, that in the end a sufficient basis should be provided for a solution of stellar problems. His practical nature led him to adopt graphical instead of analytical methods, whenever they appeared equally accurate.

Pickering loved to discuss but refused to dispute. He loved appreciation, but was not swerved from an approved course by its absence. His persistence in what he believed right was balanced by a readiness to accept new ideas. Until the very end of his life he kept an alert, unprejudiced mind, and was always glad to modify or abandon his plans if something better presented itself. He was prompt to give advice, whenever it was requested, and possibly in some cases where it was not desired. Always glad for friendly suggestions himself, he did not hesitate to offer them to others. He was held in high esteem by his fellow astronomers. The following tribute (1919) is from one of them:

"His wonderful energy and enthusiasm, his alertness, his unvarying courtesy, his wide vision and generous heart, make his passing a keen personal loss even to those of us who knew him slightly. For a number of years I have thought of him as the Dean of American Science."

Mr. Pickering was married in 1874 to Lizzie Wadsworth Sparks, daughter of Jared Sparks, a former president of Harvard University and a well-known historian. Mrs. Pickering, who is still remembered as an especially charming hostess, died in 1906. No children were born to them.

Pickering received nearly all the honors which the world had to bestow on a scientific man. These he valued highly as the expression of the appreciation in which his work was held. He received the honorary degree of Doctor from six American and two foreign universities. He took special pride in being a Knight of the Ordre Pour la Merite. His collection of medals was a large one; he was twice awarded the gold medal of the Royal Astronomical Society. In addition to membership in American societies, he was a member or associate of the national

societies of England, Germany, Ireland, Italy, Russia, Sweden, and Mexico. He was made a member of the American Academy of Arts and Sciences at the age of twenty-one, and a member of the National Academy of Sciences at the age of twenty-seven. He was President of the American Astronomical Society from 1905 until his death in 1919.

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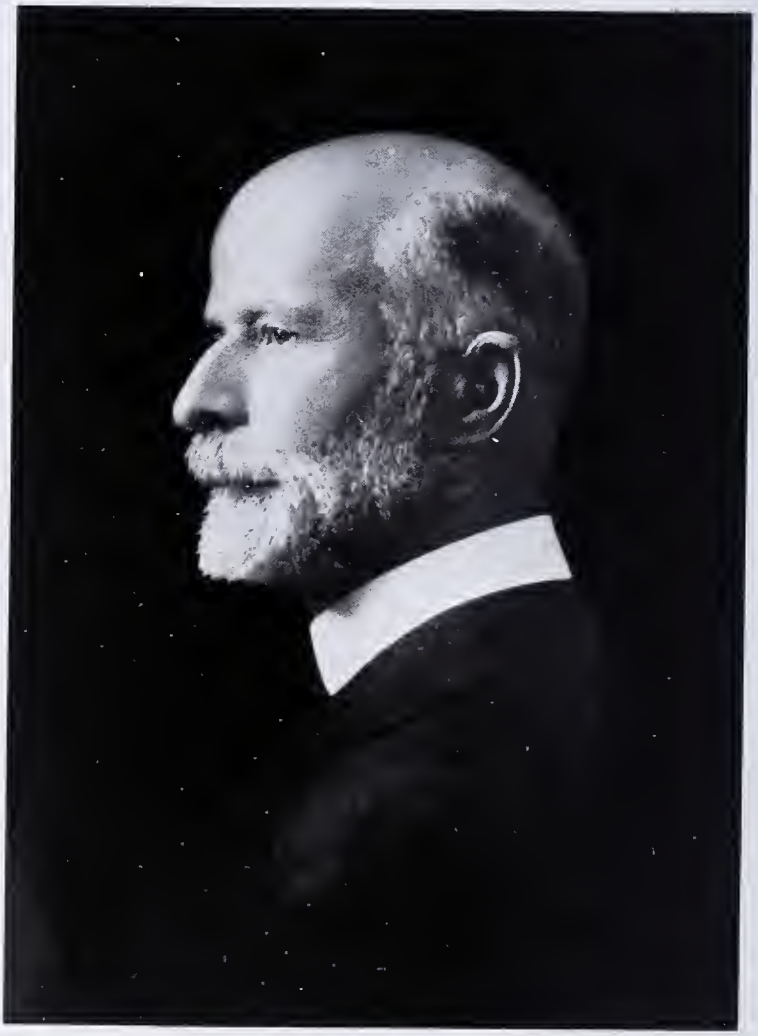
Note to Professor Pickering's Bibliography

The present list of published work does not include anything published in either the Harvard Observatory Bulletins or the Harvard Observatory Circulars. The Bulletins comprise a series of brief notes, generally on small problems or work in progress. The Circulars are a series of larger papers. In both series Professor Pickering published a great many researches; but since all Bulletins and Circulars during his directorship were printed as of the Observatory, rather than of individual members, it is impossible to tell specifically which articles were his own. Professor Pickering published also a series of Annual Reports of the Director of the Harvard Observatory to the President of the University, giving an account of the work for each year. These reports also have been omitted from the bibliography.



S. I. Bailey

NATIONAL ACADEMY OF SCIENCES
OF THE UNITED STATES OF AMERICA
BIOGRAPHICAL MEMOIRS
VOLUME XV—SIXTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

SOLON IRVING BAILEY

1854-1931

BY

ANNIE J. CANNON

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1932

SOLON IRVING BAILEY

1854-1931

BY ANNIE J. CANNON *

Solon I. Bailey, Phillips Professor of Astronomy, Emeritus, in Harvard University, died on June 5, 1931, after an illness of a few days, at his home in Norwell, Massachusetts. He had been connected for forty-four years with the Harvard Observatory, becoming Assistant Professor in 1893, Associate Professor in 1898, and Phillips Professor in 1912. From 1919 to 1921, he was also Acting Director of the Harvard Observatory.

He is survived by his wife, Ruth E. Bailey, and their son, Irving W. Bailey, professor of Plant Anatomy in Harvard University.

Born in Lisbon, New Hampshire, on December 29, 1854, Mr. Bailey, after graduating at Boston University and teaching at Tilton Academy, joined the staff of the Harvard Observatory in 1887 while studying at the University for a Master's degree. He won by his ability and excellent judgment the entire confidence of the Director, Professor E. C. Pickering, and gave him unswerving loyalty and devotion. The two men worked together in complete harmony for more than thirty years over the new projects and undertakings of the Harvard Observatory. To Bailey in 1889 Professor Pickering assigned the task of finding an elevated location suitable for the establishment of the auxiliary station made possible by the will of Uriah Boyden. Attracted by reports of the clear sky and slight rainfall on the high plateau of Peru, where also the whole southern sky is visible, he with Mrs. Bailey and their small son, Irving, sailed for Peru from San Francisco after observing the total solar eclipse at Willows, on January 1, 1889.

The story of this small expedition and their lonely life on "Mount Harvard" is told in Volume 34 of the Harvard Annals.

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* This biography was published by Miss Cannon in the Publications of the Astronomical Society of the Pacific. The bibliography was prepared by Jenka Mohr.—Harlow Shapley.

Clouds prevailed, however, for a long season, and during the second year the station was transferred to Arequipa, the "City of Sunshine." There at Carmen Alto, 8,000 feet high and two miles from the center of the town, the Boyden Station remained for thirty-five years.

With the training in photometric work already acquired in Cambridge, it was natural that Bailey's first southern investigation should be made visually. The result was that observations with the meridian photometer of 7922 stars not visible in Cambridge extended the well-known Harvard photometry to the South Pole.

However, as early as 1893, he was attracted by the accumulating photographs of the southern stars and commenced his classic study of globular clusters by a search for variable stars on excellent photographs taken with the 13-inch telescope. The field of observation was almost untrodden and remained his own for nearly twenty-five years. Fortunately he started with the finest of all globular clusters, Omega Centauri, which proved to be rich in variables. Extending the investigations to other clusters, in a few years he had discovered 509 variable stars, equaling the total number of such objects then already known.

As early as 1901 his discussion of 132 variables in Omega Centauri was published. Hundreds of thousands of measures of these faint flickering stars yielded to his careful study accurate light curves and periods. Later, the variables in the clusters M3, M5, and M15 were discussed in similar monographs. The astonishing facts were revealed that a majority of the cluster variables have periods not far from half of a day and that the median magnitude of all variables in any cluster is uniform, whatever the length of the period.

Later when it became known that the periods of Cepheids are correlated with the magnitude and indicative of the distance, these cluster variables of short period became of increased usefulness and were an important factor in Dr. Shapley's determinations of the parallaxes of globular clusters and his contributions to our knowledge of galactic dimensions.

Professor Bailey's work was characterized by thoroughness

and accuracy. Thus, after twenty years, when Dr. Shapley measured fifty-four of Bailey's variables on photographs taken at Mount Wilson with the 60-inch telescope, he found that Bailey's periods are accurate to one-tenth of a second, and added: "Few, if any, of Bailey's formulae seem to be seriously in error."

Another problem taken up in Peru was the measurement of the light of several asteroids. Eros, at the opposition of 1903, was far south. The period of variation, 0.2196 days, which Bailey derived from his extensive photometric and photographic observations, proved to be the most accurate of that time, and when two ciphers are added it has also been found to represent the light changes of 1931 to a fraction of a second.

Not all of his time in South America could be given to the stars, however, for in the absence of any reliable records a large scheme was planned to establish meteorological stations from sea-level up to an elevation of 19,000 feet. Bailey's lonely and sometimes hazardous journeys, generally on horseback, over many parts of Peru for this purpose might have daunted a less fearless man.

There were adventures such as swinging from peak to peak over a deep valley in a cage suspended by a single cable, or descending rapidly from elevations of 10,000 feet in a hand car with gravitation as the motive power, or passing nights on desolate mountains where perhaps the only sounds were the "ripple of the Rimac far below, and the flapping of a condor's wings." Several of the stations were at such high elevations that the dreaded soroche (mountain sickness) lessened his vitality so that any exertion was almost impossible.

The difficulties and discouragements of establishing a station on top of El Misti, Arequipa's nearly extinct volcano, 19,200 feet high, might have baffled a less determined man. Upon his first attempt to walk up, he became unconscious at the altitude of 16,000 feet and had to be carried down the mountain. Observing that the pack mules reached the top with slight discomfort, it occurred to him that if fatigue could be lessened by riding, this

elevated station might be maintained. Thus the problem was solved by the construction of a mule-path to the summit.

A vivid description of this project is given in Harvard Annals 39, the first volume of the Peruvian Meteorology. After Professor Bailey and his party had passed eight consecutive days and nights at altitudes from 13,000 to 19,000 feet, urging, threatening, rewarding the Indians who shoveled the trail, he thus describes the arrival at the summit: "Panting for breath, stopping to rest at every three or four steps, often struggling on hands and knees, we kept on, hardly believing there could come an end, when, suddenly, we were there. There was no introduction; we did not come to the crater; the crater had come to us. All things conspired to produce surroundings which few have seen and none described. The great altitude, the enormous craters, the sulphurous vapors, the drift clouds, the deep shadows cast by the setting sun, the inexplicable depression of spirits caused by the exhaustion and illness, combined to produce the profoundest sense of awe." El Misti station, the highest in the world, thus established, was maintained for six years, probably sufficiently long in that equable climate.

One of Professor Pickering's fondest astronomical dreams was of a "great international telescope" to be located at the best place on the planet for star study. Professor Bailey went to South Africa in 1908 to see if a better location than Arequipa could be found for this purpose. After making atmospheric tests at Cape Town, Bloemfontein, and Johannesburg, he settled upon Hanover, Cape Colony, for a temporary station and there took his beautiful long exposures of the southern Milky Way, later published in the Annals. No immediate results came from this journey, for Professor Pickering's dream never came true.

Among Bailey's honors may be mentioned that he was a foreign associate of the Royal Astronomical Society, a member of the National Academy of Sciences, the American Academy of Arts and Sciences, the Astronomische Gesellschaft, and of the International Astronomical Union, in which he was, in 1922, President of the Variable Star Commission.

In 1923, in Arequipa, he was made an honorary Doctor of

Science and honorary Professor of Astronomy in the ancient University of St. Augustin.

Wishing, as he said, one more visit to Arequipa, to settle some perplexing points about his cluster variables, Professor Bailey made plans to sail for Peru when relieved of administrative care by the appointment of Dr. Shapley as Director. It was the good fortune of the writer to go in 1922 with Professor and Mrs. Bailey on this their fifth and last expedition. Together with the delightful experiences of actually seeing celestial acquaintances of many years, such as the great star fields of Carina, the Southern Cross, and the brilliant Sagittarius rising over the Andes, was blended the pleasure of visiting the Land of the Incas with such expert guides, and of becoming acquainted with the many friends of Professor and Mrs. Bailey in Lima and Arequipa. A most cordial welcome was given them by such prominent Arequipanians as the Polars, the Romañas, the Llosas, and Mrs. Bates, of the renowned Quinta Bates. A formal reception was given by Dr. Jorge Polar in his beautiful home, and an afternoon party by the Romañas in their charming country home at Savendia, where the Peruvian national dishes were served.

Always commanding the highest respect, Professor Bailey was a great favorite in Arequipa, where he was spoken of as the ideal American gentleman. A certain New England reserve and shyness, sometimes noticeable in his own country, melted away under the tropical Peruvian sky, so that in "simpatica" Arequipa this son of the Granite State seemed to attain the finest flower of his manhood.

Mrs. Bailey was a skillful and charming hostess. Many Arequipa friends as well as American visitors enjoyed the informal teas on the Observatory balcony over the garden, constantly full of luxuriant flowers, with the beautiful view of the rushing Chile River, up to majestic El Misti, framed in the tropical sky.

But nothing in the social line was allowed to interfere with the observing. Every clear night Professor Bailey was busy as soon as darkness set in, making plates of long exposures with his

old friend, the Bruce 24-inch doublet. No matter how whimsical this instrument might be, he could nearly always coax it into good humor. Often following for six hours, during which his post could not be left for more than a minute at a time, he made many beautiful and useful plates, especially of globular clusters, the rich Sagittarius region, the almost barren south galactic pole, and the great nebulae of Carina and Rho Ophiuchi.

By day he was busy making star counts for a proposed study of stellar distribution, or in the examination of his plates for variables and nebulae. Years earlier he had been one of the first to realize the importance of the photographic study of extra-galactic nebulae. Before 1908 he had formulated a classification of "Bright Clusters and Nebulae," and had published catalogues containing nearly two thousand nebulae found on the Harvard photographs.

Modest to the extreme, never given to exploiting his own experiences or travels, unfortunately he wrote little about them except in the formal Annals of the Observatory. From year to year, on his various journeys, he made a valuable collection of Inca pottery, silver, and other historical relics of the early mythical Peruvian dwellers.

That he could write delightfully in a lighter vein is seen in occasional papers like "An Astronomical Interlude," published in the Harvard Alumni Bulletin, and "A New Peruvian Route to the Plain of the Amazon," published in the National Geographic Magazine. Buried in the meteorological volumes of the Harvard Annals are facts concerning the heights and configurations of the Andes, the volcano El Misti, the Desert of Atacama, and the Oroya Valley, which may be found useful in years to come, when, as Bailey pictured, the plain of the Amazon may be "the scene of manifold industries, and the home of millions of prosperous dwellers."

Returning to Cambridge late in 1923, he continued his investigation of cluster variables and stellar distribution, until, at Dr. Shapley's request, he started to prepare a history of the Harvard Observatory. He kept on with undiminished industry, even after his retirement in 1925, so that this useful book and also

the last instalment of Peruvian Meteorology were finished before his fatal illness. The History and Work of the Harvard Observatory, Harvard Monograph No. 4, commences even before the foundation of the Observatory with its historical background in the Massachusetts Bay Colony as early as 1674, and continues in an able and unbiased presentation of the problems and personnel of the Observatory from 1839 to 1927.

Although dignified by nature, Professor Bailey thoroughly enjoyed the human side of life, and was of such a sympathetic nature that he was often consulted on subjects decidedly outside of astronomy. He won the respect of all by his wide sympathy, his justice, his never-failing kindness, and his complete lack of self-seeking.

"His life was gentle." And yet with its gentleness and gentlemanliness, there was a great strength, the strength of faith in God and humanity, and in appreciation of the fundamental eternal truths of science and of life, for which, as he himself wrote, "there is need not only of intelligence, but of imagination."

His name should always be honorably associated with the great development of astronomy which began during the last decade of the nineteenth century, for his pioneer labors in the photography of the southern stars helped to open up the whole sky to modern astronomers, and his early investigations of variable stars in globular clusters provided an important foundation stone for the structure of the new cosmogony.

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BIOGRAPHICAL MEMOIR

OF

THOMAS STERRY HUNT

1826-1892

BY

FRANK DAWSON ADAMS

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1932

THOMAS STERRY HUNT
1826-1892

BY FRANK DAWSON ADAMS

Thomas Sterry Hunt was born in Norwich, Connecticut, on September 5, 1826, and died in New York on February 12, 1892. Forty years have now elapsed since he passed away so that but few men of science of the present generation knew him. He was, however, an outstanding figure in science in the last century and one who made many and important contributions to the knowledge of his time.

His second name Sterry was derived from his mother's side of the family. The Sterrys were a famous family among the Puritans in England in the time of the Commonwealth, several of them were preachers of renown. One of them, Peter Sterry, was Chaplain to Oliver Cromwell, while another, Thomas Sterry, was the author of the well known tract "A Riot among the Bishops, or a Terrible Tempest in the Sea of Canterbury." A branch of the family consisting of three brothers—Roger, Robert and Cyprian Sterry—together with a sister, came to America about 1753 and settled at Providence. Two of Roger's sons, John and Consider, edited and published *The True Republican,* a leading organ of the old Jeffersonian party. Jane Elizabeth, a daughter of Consider Sterry, was married to Peleg Hunt in 1823 and Thomas Sterry Hunt was their child. He often referred to his "Uncle Consider," whose name, following a common usage among the Puritans, was the abbreviation of a more extended designation, in this case a verse of Scripture, his full name being "Consider-the-lilies-of-the-field-how-they-grow Sterry." Sterry Hunt's father died in 1838, leaving his wife in rather straitened circumstances. She was, however, a woman of character and ability and by her own exertions brought up and educated her family—young Hunt had to leave school and go to work for his living at the age of thirteen. His strong predisposition toward science was manifested from the first and he contrived, while working in the shop during the day, to find leisure for study and to carry out chemical experiments.

Hunt's ideal man of science was Michael Faraday and the early career of the two men followed closely similar lines. The Sixth Annual Meeting of the Association of American Geologists and Naturalists, the progenitor of the present American Association for the Advancement of Science, which was held at New Haven in 1845, gave Hunt his first formal introduction to the world of science. He attended the meetings as a correspondent for a New York newspaper but was unanimously elected a member of the Association. Shortly after this Hunt was fortunate enough to make the acquaintance and to enlist the friendship of the elder Silliman, who played the same part for him that Davy did for Faraday. Recognizing his abilities and promise, Silliman secured his admission to the Scientific School of Yale University, where, under the younger Silliman, he entered upon his career as a chemist. During an hour or two each day he assisted Silliman in connection with his lectures, while the rest of his time was devoted to analytical work in the chemical laboratory. Writing to a friend in 1845, he says, "I have seated myself in the laboratory with the flasks by my side so as to work and write at the same time. . . . I have free access to Professor Silliman's cabinet and a key to unlock all the cases. . . . I am boarding in a club of students at $1.25 a week. We have little or no meat. I do not like this very well, but it is cheaper, though I think I will board myself after a while. The room I expected to have has been occupied, as I was uncertain whether I was coming and so I have taken up my lodgings in the loft of the laboratory building itself and am so quite at home with chemical apparatus and preparations all around, 'they are congenial spirits,' as Mr. Silliman remarked when he showed me the room."

During the years 1845, 1846 and 1847, while at Yale, he contributed no less than eighteen articles and notes to *Silliman's Journal,* his first paper being entitled "A Description and Analysis of a New Mineral Species Containing Titanium, with Some Remarks on the Constitution of Tellurium Minerals." His wide acquaintance with chemistry is also shown by the fact that he wrote the portion dealing with organic chemistry, of

Silliman's "First Principles of Chemistry." Professor Silliman in the preface to the first edition of this work writes prophetically of the high position which his young assistant was to attain in his chosen profession: "The author takes pleasure in acknowledging the important aid derived in this portion of his work from his friend and professional assistant, Mr. Thomas Sterry Hunt, whose familiarity with the philosophy and details of chemistry will not fail to make him one of its ablest followers. The labour of compiling the organic chemistry has fallen almost solely upon him."

In 1842, the Geological Survey of Canada was established and in the Spring of the following year Mr. William E. Logan (afterwards Sir William Logan) entered upon his duties as its first Director. So soon as the work of its preliminary organization had been completed, Mr. Logan felt the imperative need of an able chemist and mineralogist on his staff and wrote to Professor Silliman, Jr., asking him if he knew of anyone whom he could recommend for the position. Silliman strongly recommended Hunt as a suitable person and in this he was warmly supported by Professor C. Upham Smith and Professor J. D. Dana.

Hunt accordingly—although then but twenty years of age—came in February, 1847, to Montreal and took up his duties as Chemist and Mineralogist to the Geological Survey of Canada. For the next twenty-five years Sterry Hunt lived and worked in Canada. This was by far the most productive period of his life and his name will consequently always be associated in a very special way with the Dominion of Canada. The first report which he made after his appointment to his new position was submitted to Logan in April, 1848, and dealt with the petrography and mineralogy of the Grenville limestones occurring along the course of the Ottawa River and with certain mineral waters in this and adjacent districts. This was followed by other reports in subsequent years. An immense amount of information concerning the geology and mineral resources of the great virgin field which Canada then presented was rapidly gathered together by the officers of the Geological Survey and Logan

decided to collect and correlate this. This was done and the results were presented to the public in the large volume of nearly 1,000 pages issued in 1863 under the title of the "Geology of Canada." That portion of this volume dealing with petrography, mineralogy, the mineral waters and a large portion of the chapters on economic geology, were from the pen of Sterry Hunt.

These early years on the Canadian Survey brought to Hunt a great wealth of information and directed his attention to certain lines of thought and investigation which he developed as the years went on. While he undertook a certain amount of geological field work and made a number of important observations in the field, and while he wrote extensively on geological subjects, he was first and foremost a chemist and he approached all the subjects which he studied from the standpoint of chemistry. His contributions to geology dealt chiefly with the old crystalline and metamorphic rocks of the earth's crust.

His work, however, on the structure of the palaeozoic rocks of Ontario and of the Gaspé Peninsula and its relation to the occurrence of petroleum was of the first importance. In a paper entitled "Notes on the History of Petroleum or Rock Oil," which appeared in the *Canadian Naturalist and Geologist* for August, 1861 (Vol. VI, pp. 241-255), referring to a large number of oil wells which had been sunk in the township of Enniskillen in the Province of Ontario, Hunt writes: "These wells occur along the line of a low broad anticlinal axis which runs nearly east and west through the western peninsula of Canada . . . the oil doubtless rises from the Corniferous limestone which contains petroleum, this being lighter than the water which permeates at the same time the porous strata, rises to the higher portion of the formation which is the crest of the anticlinal axis, where the petroleum of a considerable area accumulates and slowly finds its way to the surface through vertical fissures in the overlying Hamilton shales, giving rise to the oil springs of the region." Earlier in the same year, in a lecture which he delivered before the Board of Arts of Montreal and which was reported in the *Montreal Gazette* of March 1, 1861, Hunt

pointed out this relation of the oil wells of Ontario to the anticlinal folds in this region. This relation of petroleum deposits to anticlinal folds, the recognition of which has led to such tremendous economic results, Hunt believed that he was the first to point out in the letter in question. In which belief he was apparently correct. Professor E. B. Andrews, in a paper on "Rock Oil, Its Geological Relations and Distribution," in the *American Journal of Science* for July, 1861, describes the occurrence of oil and gas springs on the axis of an anticlinal fold in Ohio. In his pamphlet "Petroleum, Its Geological Relations," published in Quebec in 1865, Hunt then acting as Director of the Geological Survey of Canada, Sir William Logan being absent from Canada at the time, makes a somewhat detailed report of the occurrences of petroleum in the Gaspé Peninsula in the Province of Quebec in which these petroleum occurrences also are shown to be directly connected with anticlinal folds, the oil rising from petroleum-bearing strata below into a pervious overlying series of beds with an impervious cover.

The wide range of researches and the extended contributions which he made to science will be seen by glancing over the bibliography which is appended. It is unnecessary to refer to all these but there are two subjects to which he devoted much time and study and to which attention may therefore be directed.

The first of these is that of the "Origin and Succession of the Ancient Crystalline Rocks of North America."

In his "Chemistry of Metamorphic Rocks," read before the Dublin Geological Society in 1863, he classified these into the following four series set forth in ascending order: 1. "The Laurentian System representing the oldest known rocks and supposed to be the equivalent of the Primitive Gneiss formation of Scandinavia and that of the Western Islands of Scotland." 2. "A great development of anortholites of which the hypersthenite or opalescent feldspar-rock of Labrador may be taken as a type. These strata overlie the Laurentian gneiss and are regarded as constituting a second and more recent group of crystalline rocks, to which the name of the Labrador Series may be provisionally given. From evidence recently submitted, Sir

William Logan conceives it probable that this series is unconformable with the older Laurentian System and is separated from it by a long period of time." 3. "A great series of crystalline schists (the Green Mountain Series) which in Canada are referred to as the Quebec Group, an inferior part of the Lower Silurian System. It appears to correspond both lithologically and stratigraphically with the schistose group of the Primitive Slate Formation of Norway. The Huronian series of Canada in like manner appears to correspond to the Quartzose Group of the Primitive Slate Formation. It has as yet been but imperfectly studied and will not be further considered." 4. "This is a series of quartzose and micaceous schists with Staurolite and red garnet," which he named the White Mountain Series.

He states in this paper that it cannot be doubted that in the earlier period of the world's history, chemical forces of certain kinds were much more active than at the present day. In the successive periods above mentioned, he believed that owing to successive chemical changes in the character of the ocean waters and of the detritus carried into the oceans from the land, the deposits laid down on the ocean floor—partly chemical precipitates and partly fine clastic material—were characterized by a progressive change in chemical composition, which when the deposits were metamorphosed gave rise to the distinctive differences in petrographical character displayed by the four series referred to above. He goes on to say that the question therefore arises whether the chemical conditions which have presided over the formation of sedimentary rocks have not so far varied in the course of ages, as to impress upon the rocks marked chemical and mineralogical differences, which in the absence of organic remains or stratigraphical evidence might be used as a means of determining the age of a given series of stratified rocks.

In his address as Vice President of the American Association for the Advancement of Science in 1871, he set forth a revised and extended classification of these ancient rocks, now arranging them in the following six series, again in ascending order: Laurentian, Norian, Huronian, Montalban, Taconian, and Keewenian.

His opinion both as to origin and succession of these ancient rocks was opposed by Dana, Logan and others and led to widespread controversy. His views as to their origin inclined largely to those of the old Neptunists and have not received much support from more recent studies. The Norian series has been shown to have no place in the succession, being composed entirely of intrusive rocks and many of the rocks which Hunt regarded as of sedimentary origin and characteristic of the other series have proved to be of igneous origin. He, however, carried on a vigorous campaign in support of his views to the end.

The last field work which Hunt did was in southeastern Pennsylvania in the years 1875 and 1878. This was for the Second Geological Survey of Pennsylvania under J. P. Lesley. He was to have written two volumes for this Survey, the first being an historical account of the development of the knowledge of "Azoic" or Pre-Cambrian rocks of North America, while the second volume was to have given an account of the rocks of this age in Pennsylvania. Only the first of these volumes was completed. It was published in 1878 under the title of "Special Report on the Trap Dykes and Azoic Rocks of Southeastern Pennsylvania." Professor Lesley in his introduction to this volume says: "It is a treasury of notes and suggestions of the greatest value to the geologists of Pennsylvania and of other States working in such districts as are occupied at the surface or are underlain at moderate depths by the Cambrian and sub-Cambrian formations, although no final demonstration has been accomplished by the author of those problems of superposition, unconformability and identification at which so many geologists are still half despairingly at work. But his opinion of the final solutions of these problems will reinforce their own when they agree and lead to discussion when they disagree." The very difficult problem of deciphering these half-obliterated palimpsests in which is set forth the history of the primeval world, cannot indeed be solved by any amount of discussion but only by long-continued, close and accurate observation in the field—a method of approach which, it is feared, was not congenial to Hunt.

Fourteen years later, when in 1892 Lesley presented a compendium of the results attained by the Second Survey of Pennsylvania in his "Summary Description of the Geology of Pennsylvania," the somewhat transcendental character of Hunt's geological studies and opinions seems to have impressed him, for in his chapter on the "Earliest Archæan" in Volume 1, he writes as follows:

"The surface of the earth was no longer hot enough to keep all the water of the planet in a state of vapour in the surrounding atmosphere; descending in local deluges of sour rain to boil upon the rocks and dissolve apart their mineral elements, sweep them into hollows and there leave them while it sprang aloft as steam to rejoin the universal canopy of cloud. All of this had taken place before the first age of which we have any geological monuments and is known only to God and Dr. Sterry Hunt, who has described it magnificently in his 'Chemical Researches'."

These views of Hunt referred to by Lesley are put forward in his paper on "The Chemistry of the Primeval Earth," which is to be found in his "Chemical and Geological Essays," published in 1874 and which volume of essays passed through four editions. This essay was developed and expanded in his later paper entitled "The Origin of Crystalline Rocks," which appeared in the "Transactions of the Royal Society of Canada," Section III, 1884. This paper is an exposition of his "Crenitic Hypothesis."

According to this theory a basic quartzless rock formed the exterior portion of the cooling molten globe and was the last portion of the globe to solidify. This was fissured and made porous through refrigeration and crystallization and was thus rendered permeable to considerable depths. The heated waters which condensed upon this surface thus passed downward but owing to the central heat of the earth, a system of aqueous circulation was set up in the mass. The materials which these heated waters dissolved out of this basic rock through which they were circulating would be essentially those which infiltrating waters in subsequent ages have removed from erupted masses of similar rock, namely silica, silicates of the alkalies and of lime and cer-

tain double silicates of these bases with alumina (zeolites and feldspars), together with oxides of iron and carbonate of lime, due to admixture of atmospheric carbon dioxide. These materials would be brought to the surface and there deposited, or they might be deposited in the fissures through which the waters rose. In fact they are, according to this theory, to be seen now in the pegmatite veins or dykes cutting the most ancient rocks of the earth's crust in all parts of the world where these are exposed. These waters, however, when they reach the surface would deposit their dissolved contents, as the temperature was lowered, in layers of crystalline acid granitic or gneissic rock, spread over the original surface of the cooling globe while the insoluble basic residue of the original crust represents the typical basaltic layer assumed by Bunsen.

Therefore all gneisses, hornblende and micaceous schists, olivine rocks, serpentines and in short all crystalline stratified rocks, are of neptunian origin and not the results of the metamorphism of either aqueous sediments or of volcanic materials. The chemical and mechanical conditions under which these rocks were deposited have not, according to Hunt, been reproduced since the beginning of Palaeozoic time. The eruptive rocks, according to this Crenetic Hypothesis are for the most part softened and displaced portions of these ancient neptunian rocks or of the basic shell on which they rest.

In another of his more important papers, entitled "On a Natural System in Mineralogy with a Classification of Natural Silicates," Hunt gives expression to certain ideas on another subject, which had been maturing in his mind for several years. This was read before the Royal Society of Canada in 1885 and printed in the "Transactions" of this Society in the following year.

Werner, who died in 1817 and was succeeded at the Freiberg School of Mines by Frederich Mohs, prepared a classification of the Mineral System based on external characters. Mohs in his "Mineralogy," which appeared in 1822-24, completed Werner's work and the method of classification thus introduced was designated by Jamieson as the "Natural History Method" for

the classification of minerals. Dana adopted this method in the first and second editions of his "System of Mineralogy," but abandoned it in the third edition published in 1850, and in the preface to the fourth edition stated that in his opinion the system of Werner and Mohs had subserved its end and that a system more consistent with the new views on chemistry should be adopted. He accordingly, in his fourth edition, which appeared in 1854, adopted a chemical classification, the so-called Berzelian System.

In this paper Hunt proposes a system of classification in which he combines these two methods, holding that the physical features of minerals "had inherent and necessary relations with the chemical composition," and this system he therefore calls a "Natural System of Mineralogy." As organic chemistry is the chemistry of the carbon compounds, so Mineralogy is largely the chemistry of the compounds of Silicon, and in this new system Hunt strives to show that the structure of the silicate molecules in minerals is analogous to that found in the carbon compounds. "All species crystallizing in the same shape have the same equivalent volume so that their equivalent weights (as in the case of vapours) are directly as their densities and their equivalents of mineral species are as much more elevated than those of the Carbon Series as the specific granites are higher. . . . The laws of progressive or homologous series, previously recognized only in hydrocarbonaceous bodies, must be extended to mineral species and are of universal application. . . . The variations observed in chemical composition of mineral species are due not only to their high polybasic character, but also, in certain cases to indefinite admixtures of homoeomorphous species, as previously indicated by Delesse, Scheerer, and Von Wathershausen, extended and generalized by myself and subsequently adopted by Tschermak."

Hunt's "Crenitic Hypothesis" and his "Natural System in Mineralogy" have not met with very wide acceptance, but time will show whether, with the advance of science, they may not be found to contain some things which are worthy of more attention than they have hitherto received.

In addition to the immense amount of detailed analytical and descriptive work which Hunt carried out as Chemist and Mineralogist of the Geological Survey of Canada, he gave a course of lectures on Chemistry every Spring at Laval University in Quebec, from 1856 until 1862. These lectures he delivered in French, which language he spoke with fluency and elegance. He also for several years lectured at McGill University in Montreal. It was not, however, until he severed his connections with the Geological Survey of Canada to accept the Professorship of Geology in the Massachusetts Institute of Technology, in 1872, that he devoted his time exclusively to university work. He retained his professorship in Boston until the year 1878. Teaching, however, was not congenial to him. He had neither the desire nor the patience to follow what were to him the dry details of the geological study of periods of the earth's history in which he took no particular interest. Neither had he the peculiar qualities of personality which lead the teacher to take a special interest in his students, which in the case of so many teachers call forth a peculiar spirit of devotion to him on the part of his students. His talents lay in other directions, and so later his application for the Chair of Geology at Columbia University, although supported by Lyell, Murchison, Dana, Silliman, the Rogers and others, was rejected in favor of Newberry.

While devoting much of his time to problems of pure science Dr. Hunt was also attracted by chemistry and geology in their practical applications. When on the staff of the Geological Survey of Canada he made reports on the "Gold Mines of Canada" (1863); "On Petroleum in Gaspé" (1865); "On the Gold Regions of Lower Canada" (1866); "On the Gold Deposits of the County of Hastings, Ontario" (1867); "On the Goderich Salt Region in Ontario" (1869), etc., and wrote many other papers dealing with economic geology.

In 1857 he first suggested the use of sesquioxide of chromium as the basis of a green ink for printing bank notes, it being a substance which could not be removed by either an acid or an alkali without destroying the paper and which also could not be satisfactorily photographed. This invention was patented and

being subsequently adopted in the United States for the printing of "greenbacks" became very valuable, but Hunt himself never reaped any substantial reward from his discovery.

Early in his career he became acquainted with Dr. James Douglas, of Quebec, an acquaintance which ripened into an intimate and lifelong friendship. Dr. Douglas, who later became associated with the celebrated Copper Queen Mine in Arizona, was at this time interested in the development of certain copper deposits in the Province of Quebec and with Hunt invented the Hunt and Douglas process for the extraction of copper by a wet method. This was described in a paper which appeared in 1871. Dr. Douglas read before the American Philosophical Society in 1898 an extended obituary notice of his friend Sterry Hunt, giving many details concerning his early life work which have been drawn upon in preparing the present memoir.

Dr. Sterry Hunt had an outstanding personality. He came of good stock and had cultivated his great natural abilities. Not only had he an intimate knowledge of those branches of science in which his work chiefly lay, but he possessed a wide general knowledge of the other natural and physical sciences. He had furthermore a wide acquaintance with literature and a highly cultivated literary taste. He had a peculiar charm of manner and was an excellent speaker and a brilliant conversationalist, conversations with him, however, generally passing into a monologue on his part, to which the other members of the party became interested and often delighted listeners. He always wrote and spoke with accuracy and, where the subject permitted, with elegance. He had a perfect command not only of English but also of the French language, speaking French readily and having a wide familiarity with French literature. The Abbe J. C. K. Laflamme, of Laval University, who knew Hunt intimately, in his presidential address to the Royal Society of Canada in 1892 writes of him "Il garda cependant toute sa vie un veritable culte pour la forme. On le constate facilement en parcourant n'importe lequel de ses ouvrages. La phrase est tonjours irreproachable et facile, souvent élégante."

He impressed the writer, who, when a student at McGill

University often met him, as a man of real genius; but if he was such it was not that "genius which is patience," according to Buffon's definition, for Hunt was somewhat impatient and sometimes even irritable in disposition. His mind, active and speculative, tended to pass rapidly from one great conception to another and would not brook the delay necessary to examine details which were often vital to the subject.

A characteristic which impressed many people unfavorably was his rather too frequent and definite insistence on priority for his many views and opinions. It is to be feared that, like some other great men, he possessed, together with other undesirable qualities, a certain amount of vanity in his disposition. Dr. Douglas in his memoir of Hunt, to which reference has been made, says of Hunt:

"His temperament was always distinctly religious. He went to Canada as a high-strung, imaginative boy, who had been brought up in the strictest school of Connecticut Congregationalism. In Montreal he was at once admitted into the inner circle of the French Canadian Society, which retained much of the culture and grace of the Ancien Régime, was devoutly Catholic, and was controlled by French ecclesiastics of great suavity, tact and intellectual acuteness. Under these influences Hunt adopted the Roman Catholic faith, and remained a devout son of the Church till after the breaking out of the war of Rebellion, when he abandoned the Church as openly and with the same courage and sincerity as he had shown in entering it, though in so doing he alienated some of his dearest friends. Whatever may have been his faults, cowardice and duplicity were not among them."

Hunt received many honors, degrees and decorations. The degree of M. A. was conferred upon him by Harvard University. The University of Cambridge, England, gave him the honorary degree of LL. D. in 1881, assigning to him while its guest, chambers in Trinity College near the rooms which had been occupied by Newton. It was under the inspiration which he felt on this occasion that he wrote his admirable essay "On Celestial Chemistry from the Time of Newton." Laval Univer-

sity conferred upon him the degree of LL. D. at its first convocation. He was elected a Fellow of the Royal Society of London in 1859, being the youngest man who up to that time had received this coveted distinction. In 1873 he was elected to membership in the National Academy of Sciences. He was also a Chevalier of the Legion of Honor and subsequently was raised to the rank of Officer.

After the Congress of Bologna, King Humbert decorated him with the Order of St. Mauritius and St. Lazarus. He took an active part in founding the Royal Society of Canada in 1882. Hunt was the first Chairman of the Section of Mathematical, Physical and Chemical Sciences and was elected President of the Society for the years 1884 and 1885.

Hunt remained a bachelor until late in life, but in 1877 married Miss Anna Rebecca Gale, who belonged to a very well-known family in Montreal.

In the early eighties his health began to fail and the last two years of his life were spent in his room at the Park Avenue Hotel, New York, or in St. Luke's Hospital. He, however, worked continuously till the very last and up to the day before he died spent several hours each day at his desk engaged in the preparation of a new book. During these last dreary months of confinement his chief solace was in tending his flowers and plants, in which he took a deep interest, not only as a well-trained botanist but as a lover of all things of beauty, having probably in mind the well-known words of St. Francis:

Laudato si, mi Signore, per sora nostra madre terra
La quale ne sustenta et governa,
Et produce diversi fructi con colorite fiori et herba.

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Abbreviations

A. A. P.—American Academy Proceedings.
A. C.—American Chemist.
A. N.—American Naturalist.
B. A. R.—British Association Reports.
C. & G. E.—Hunt's Chemical and Geological Essays.
C. G. S.—Canadian Geological Survey.
C. J.—Canadian Journal.
C. N.—Canadian Naturalist.
C. News—Chemical News.
C. R.—Comptes Rendus, Paris.
D. G. S. J.—Dublin Geological Society Journal.
E. & M. J.—Engineering and Mining Journal.
Erd. J. P. C.—Erdman Jour. Prakt. Chem.
Geol. Mag.—Geological Magazine.
I. M. E.—American Institute of Mining Engineers Transactions.
M. P. & P.—Hunt's Mineral Physiology and Physiography.
P. A. A.—Proceedings of American Association.
P. M.—Philosophical Magazine.
Q. J. G. S.—Quarterly Journal of the Geological Society of London.
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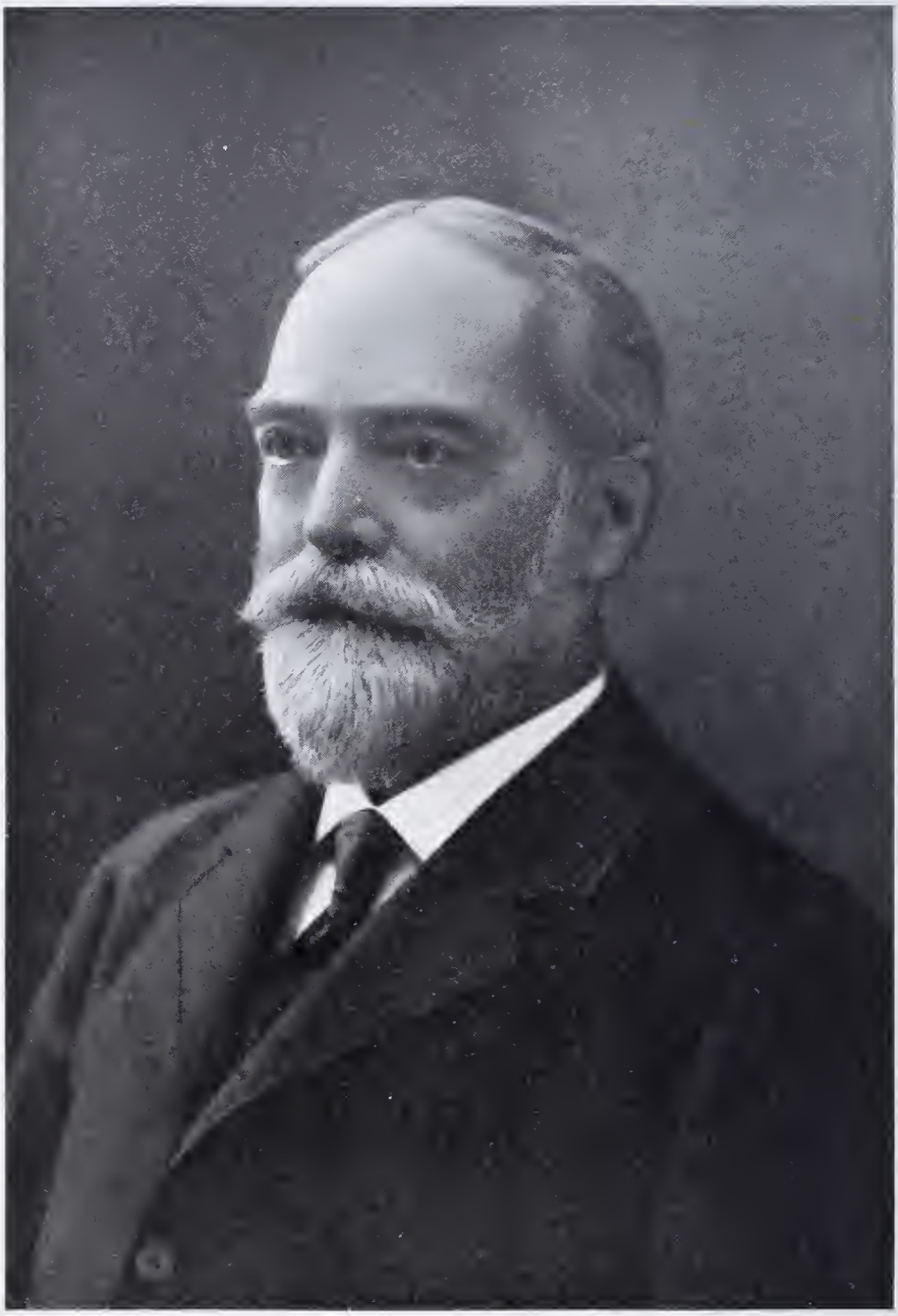

Arthur W. Wright.

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OF

ARTHUR WILLIAMS WRIGHT

1836-1915

BY

EDWARD S. DANA

PRESENTED TO THE ACADEMY AT THE ANNUAL MEETING, 1932

ARTHUR WILLIAMS WRIGHT

1836-1915

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The quiet life of a student of science often presents few incidents of striking prominence. As a college professor his first work is that of teaching—in a word, presenting to his students the essential principles of his subject and awakening their interest in them. Further, in order to keep in touch with the progress being made in his department, he must, in his free time, make a close study of the growing literature of his subject. This will also suggest to him the directions in which his own investigations can best be extended.

The many years that Professor Wright spent at Yale were lived essentially along these lines. He was a man of modest deportment, but cordial and pleasing approach. He was faithful to his students in the class and lecture-room, presenting his subject with the clear directness of a well-trained scientist. Of the scientific literature, which more and more crowded the library, he was ever a constant reader. Thus his mind became stored with the facts of his subject. Also, he was keen to note any signal steps of progress and to direct his experiments in that direction. His early study of Röntgen rays, noted later, illustrates this point. He was also the first in New Haven to set up a private telephone line between his home and the laboratory.*

While it cannot be claimed that Professor Wright opened up and developed any striking line of progress in physics, it is true that his many experimental investigations were important and all on a high level. Also, independent of this, it is interesting to note that his coming to Yale resulted in two important steps of progress for the University and for science as well. For this Wright should be long remembered. It seems well to explain these points in detail before considering the events of his life and the results of his work in the laboratory.

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*The first commercial telephone exchange in the world was established in New Haven on January 28, 1878.

In the first place, it may be fairly said that Yale owes to Professor Wright the possession of a well-planned and fully equipped physical laboratory. It is probably true, also, that the Sloane Physics Laboratory, completed in 1883, was the first building in this country which was exclusively devoted to the work of modern physics. Further, this early laboratory led directly, as will be shown later, to the University Sloane Laboratory, opened in 1912, which in size, equipment, and endowment is all that a university laboratory should be. For this result, Yale certainly owes a large debt of gratitude to Professor Wright. This will be made clear by the explanation which follows.

The fact that the donors of the Physics Laboratory of 1883, Mr. Henry T. Sloane (Yale 1863) and his brother, Mr. Thomas C. Sloane (Yale 1868), were led to make this generous gift, is explained by the fact that they were close personal friends of Professor Wright. Also, it was due to the careful study given by Wright to the maturing of the plans that this laboratory, in arrangement and equipment, was fully up to the requirements of the time. In this building there were carried on for many years the instruction, study, and experiments of Professor Wright and his younger associates; also of Professor J. Willard Gibbs, who was appointed to the chair of mathematical physics in 1871. It is interesting to add that Mr. Thomas C. Sloane, who died in 1890, left as a bequest to the Sloane Laboratory the sum of $75,000, the income of which was to be used for research.

Much later, about 1908 or a little earlier, the subject of a new, much larger, and thoroughly modern, laboratory of physics, was raised by Mr. Henry T. Sloane. It was his wish, as it was that of the members of the faculty with whom he conferred (conspicuously Professor Bumstead) that in this new building the physics of all the departments of the university should be brought together. This plan was carried through and thus the first step, and that a vital one, was taken in uniting the work of the college with that of the Sheffield Scientific School and graduate school. The new Sloane Laboratory, it should be added, was the first of the group of university buildings erected on what is now known as the Pierson-Sage Square.

The funds needed for the construction and endowment of this new Sloane Laboratory were given by Mr. Henry T. Sloane and his elder brother, Mr. William D. Sloane. The cost of the building was $385,000, and an additional sum of $125,000 was given for endowment. The work of developing the plans was largely in the hands of the late Professor Henry A. Bumstead, who was also made the Director of the new laboratory in 1909. Professor Bumstead, it should be added, was a man of rare ability and attainments. His early death (December 31, 1929) was a great loss to his department and to Yale.

In another direction Wright's appointment as professor of physics marked an important step forward in the position of this subject. Until then, at Yale and at most other institutions in the country, the subject now known by the simple name of physics was included under the general head of natural philosophy. With this was commonly linked, in the title of the professor, either astronomy, mathematics, or chemistry, or two of these.

Thus Denison Olmsted (Yale 1813) was professor of mathematics and natural philosophy (also later of astronomy) from 1825 to 1859. His successor, Elias Loomis (Yale 1830), the astronomer and meteorologist, was professor of natural philosophy and astronomy from 1860 until 1889. Wright himself was tutor of natural philosophy in 1866-68, and in 1875 he was made professor of chemistry and molecular physics. Later, however, it was decided that in general one subject was all that a given professor should be asked to be responsible for, and in accordance with this, Wright ceased to teach chemistry in 1879. This was an important step in the handling of the sciences. More noteworthy than this is the fact that Wright's chair was the first in which the subject, hitherto vaguely defined as natural philosophy, was definitely called physics. In a word, Wright was the first professor of physics at Yale.

It may be added, however, that natural philosophy long held a place in the Yale Catalogue. It was altogether right that the chair of Professor Loomis should retain its original name of Munson professor of natural philosophy and astronomy from

1860 to 1889. But it was, however, strange that in 1879, several years after Wright was made professor of physics, an assistant professor of "natural philosophy" should have been appointed.

It is also interesting that, from the early days through 1872, the text book used was Olmsted's "Natural Philosophy." Even in 1872-73, when Ganot's "Physics" was named as the text book, the subject was still called natural philosophy in the Yale Catalogue. It was not until 1883 that physics fully took its place. This change is dwelt upon at some length because so intimately connected with the career of Arthur Wright.

A digression may be pardoned here, to make clear the place occupied by Wright in the wide development of science at Yale. The starting point is properly taken as 1802, when President Timothy Dwight appointed Benjamin Silliman (Yale 1796) professor of chemistry and natural history. Silliman was then a young man of twenty-three, planning for himself the career of a lawyer, which plan, however, he gave up when he decided to join the Yale faculty.

This step taken by President Dwight was especially noteworthy, because so far-reaching in its results and taken at so early a date. It is also interesting that it was made by a president who, although a Congregational clergyman (as were all the leaders of Yale from 1701 to 1899), had none of the feeling, which later developed here and there, that the teachings of science and those of religion were essentially more or less antagonistic.

The appointment of Silliman had a marked effect in increasing the prominence of science at Yale and also over the whole country. In the latter direction, the influence of the man himself was felt especially through his frequent lecture courses in many prominent cities. Having gained his scientific training chiefly abroad, the new professor assumed his duties at Yale. His laboratory was the Old Commons Hall of 1782, refitted for its new use in 1819. Silliman worked actively in chemistry until his retirement in 1853. His son, Benjamin Silliman, Jr., became his assistant in 1837 and later his successor, and carried on for a number of years the lectures in chemistry. In 1850, James D. Dana (Silliman's son-in-law) became professor of natural his-

tory and in 1864 professor of geology and mineralogy. Thus science developed at Yale.

In the meantime, the field of physical science, which opened with the work of Denison Olmsted and his successor, Elias Loomis, was firmly established by Wright, the department being named physics as has been already explained. Wright also gave at first, as had Silliman, Jr., lectures in chemistry. Silliman, Jr., and Norton, however, as early as 1847, gave instruction to students in applied chemistry with laboratory work. For this, at the outset, the early president's house of 1797-99 was used; the site of this building was that now occupied by Farnam Hall. This date of 1847 may be taken as that which marks the first of the successive steps which led up to the beginning of the Yale Scientific School. This school, founded by the intensive work of Brush, Norton, Brewer, and others, developed into an independent department of Yale, most important at home and in its influence on broad scientific instruction in the country. It became the Sheffield Scientific School in 1863, then named after its chief donor, Mr. Joseph E. Sheffield.

In the college, chemistry held but a small place from Silliman's time until the Kent Laboratory was built in 1887. This building was given up to other departments much later when the Sterling Chemistry Laboratory on the Pierson-Sage Square was completed. Here the work in chemistry of Sheffield Scientific School, the college, and the graduate school are united, as had been that in physics much earlier in the adjacent Sloane Physics Laboratory of 1912. With this second decisive step, the university development of Yale in science was fully established.

It is not amiss to state that Yale might have claimed to be a university very early, when the three departments of medicine, theology, and law were established in 1810, 1822, and 1824, respectively. The name, however, was not used until 1887, though the graduate school was established in 1847.

The life of Arthur Wright began at Lebanon, Connecticut, on September 8, 1836. His early ancestors on both sides were of good English stock, coming to New England in the very early years of the new Colony of Massachusetts. His father, Jesse

Wright, was a man of prominence in the state as a member of the Connecticut House of Representatives in 1839. Also in his native town of Lebanon he was Justice of the Peace and later (1840-42), Selectman. On this side the Wright family goes back through eight generations to Samuel Wright, who settled in Springfield in 1639. Arthur Wright's mother was Harriet Williams, the daughter of William Williams of Lebanon. Her earliest ancestor in this country was Robert Williams, who was born in England in 1593, but in 1637 made his home in Roxbury, Massachusetts.

Wright received his early education in Lebanon, but his special preparation for college he obtained principally from Bacon Academy, Colchester, and later at Kinne's private school in Canterbury, not far distant from his native town. He entered college in 1855 and through his course maintained an excellent rank as a scholar, obtaining at graduation in 1859 the stand of High Oration. He did especially well in Latin, also in mathematics and astronomy. Further than this, he stood well with his classmates on the personal side. His facility in music was a point in his favor, and gave him a place in several musical societies. His continued interest in music contributed much to his happiness throughout life.

The class of 1859 numbered at graduation 107 men. It was a notable company, including several who also attained positions of special prominence. The following deserve to be mentioned: John Haskell Hewitt, a student of the ancient languages and professor in Williams College from 1882 to 1909; Thomas Raynesford Lounsbury, who will be long remembered as professor of English in the Sheffield Scientific School; William Thompson Lusk, a prominent physician, connected with Bellevue Hospital from 1864 till his death in 1897; Eugene Schuyler, the diplomat, Consul General at Constantinople in 1878, and United States Minister to Roumania, Servia, and Greece from 1882-1884; also Joseph Hopkins Twichell, the distinguished clergyman and writer in Hartford, Connecticut.

After receiving his bachelor's degree, Wright continued his studies at Yale in the recently organized graduate school, where

he specialized in mathematics and science. In 1861, he and two others were the first to receive the degree of doctor of philosophy in the recently established department of philosophy and the arts. Wright's comrades in this honor were his classmate, Eugene Schuyler, spoken of above, and James Morris Whiton, Yale 1853, who was for years rector of the Hopkins Grammar School in New Haven, which was founded as early as 1860. During this period, Wright was assistant in the Yale College Library; also librarian of the Linonian Library from 1860-63.

Wright's connection with the Yale faculty began with his appointment as tutor in 1863. This name was that long used for an instructor, but practically dropped after 1905. A tutor in the early days, and down to 1880 or later, was usually called upon to teach one of the fundamental studies of the curriculum, Latin, Greek, or mathematics. As between several tutors appointed in a given year to teach one of the lower classes, the order of choice of subjects was made for each according to the year of his graduation. In other words, a junior tutor might find that the subject left for him was one in which he had only a minor interest. For example, of the tutors chosen from a class in the 1870's, one of these, though particularly proficient in Greek, was compelled, to his sorrow, to teach mathematics.

Wright was fortunate in this last particular, for he was tutor in Latin from 1863-66; this was a subject, as already stated, in which he had excelled when an undergraduate. In 1866 and 1867, he was tutor in natural philosophy. It is interesting also that, while tutor, being uncertain as to his future, he pursued the study of law and was admitted to the bar, though he never earned his living by actual law practice. This course was not exceptional, it was also true of Benjamin Silliman in 1801. The years 1868-69, Wright spent in Europe, continuing his life there as a student at the University of Heidelberg under Bunsen and Kirchhoff, and at Berlin under Hofmann and Magnus. Returning to this country, he continued his career as a teacher when made professor of physics and chemistry at Williams College in 1869; there he remained until 1872.

The year of 1872 stands out prominently in Wright's life, for after the three years at Williams College, he was called to Yale as professor. His title, as given in the Catalogue, was at first of chemistry and molecular physics. In 1876, this was changed to molecular physics and chemistry, which it remained until 1887, when the chemistry was dropped and his chair became that of experimental physics. He was, however, relieved from the teaching of chemistry for some years before 1887. His activity as professor of physics continued until 1906, when he became emeritus.

Wright's work at Yale as professor of physics was at first carried on in the aged laboratory already spoken of as used by Benjamin Silliman early in the century. This was a quaint structure, for the most part of one story only, with a long, sloping roof. The laboratory was planned chiefly for the lectures in elementary chemistry. There was one tier of steeply sloping seats for the Yale students, another small tier at right angles to the other and hence partly concealed, which was used in Silliman's time for privileged young ladies. This laboratory provided scant facilities for advanced work and original investigations. Its limitations were especially trying to Wright, the physicist, which makes it remarkable that he could have accomplished as much as he did, as shown in the bibliography of his papers, given later.

From this early Silliman laboratory, Wright moved to the Sloane laboratory in 1883, mentioned earlier, where all the problems of arrangement and equipment had been satisfactorily solved in advance by himself. His labors were increased, however, since for about ten years the lectures and the laboratory instruction were carried on by himself. In the early 1890's he was relieved of all but the instruction of special students in the laboratory. As already stated, he became emeritus in 1906. This event was determined by age, according to the custom adopted at Yale about this time.

Wright's personal life was that of a solitary bachelor at Yale from 1872 to 1875; during this interval he resided in the dormitory on the Yale Campus named Durfee College, after the donor,

Bradford Durfee (Yale 1867). After October 7, 1875, his life became a happier one, for on this date he was married to Miss Susan Forbes Silliman, the eldest daughter of Benjamin Silliman, Jr. Their life together continued until February 17, 1890, when Mrs. Wright died. His own death followed much later, on December 19, 1915.

The Wright's had three children: Susan Silliman Wright, now Mrs. Winchester Bennett; Dorothy Silliman Wright, Mrs. Edwin Pugsley; and Arthur Silliman Wright. These all reside in New Haven, and in the next generations there are (1932) nine grandchildren and two great-grandchildren.

It may be said in general that Wright's original work in science was for the most part in physics, although this was often allied with chemistry. It remains to present an account of its value and scope. This can best be done by quoting from a paper by his colleague, the late Professor Hastings, which was published in 1916. This gives an appreciative estimate as to the character of his work followed by an account of the more important of his investigations. The titles of the papers referred to are given in the Bibliography, which closes this notice.

It may be added here that Wright was much aided in the study of the literature of physics by his command of German, acquired when a student in Germany; also by his reading knowledge of Danish and Italian, which he gained by private study. In his experimental work, his skill in glass blowing was an important element.

Professor Hastings says: "His contributions are not merely important in substance; they are also characterized by unusual excellence of form and clarity of statement. His fertility in ideas, his full knowledge of the literature of his subject and, not the least, his rare manual dexterity, together enabled him to make a distinguished record as a physical investigator.

"An early paper, published in 1870, was entitled 'On a peculiar form of the discharge between the poles of the electrical machine.' This paper describes the glow produced upon the positive ball in an active electrical machine and the conditions under

which it may be produced. The striking fact that each portion of this luminous surface can be regarded as due to the effect of a point area on the negative ball, as proved by sharp geometric shadows formed by minute obstacles anywhere within the region between the conductors, is quite new and it affords a particularly beautiful method of determining the shape and position of the lines of force. This investigation was followed by another, 'On certain forms of the electrical discharge in air,' which contains extensions of experimental methods and results attaching to the positive glow described in the previous paper.

"A description of a simple apparatus for the production of ozone was followed by two studies of the chemical action of ozone. The first of these—'On the action of ozone upon vulcanized caoutchouc' (1872)—calls attention to the cause of the deterioration of the insulating properties of vulcanite and gave means of correcting the fault. The second paper is entitled, 'On the oxidation of alcohol and ether by ozone,' and is an application of his ozone apparatus to the chemical investigation indicated in the title.

"In the same year Professor Wright published two papers on the Zodiacal Light of Coggia's Comet. In the first of these papers the question of the polarization of the Zodiacal Light, even to a fair determination of the ratio of polarized light to unmodified light, seems to have been definitively settled by the skillful use of a polariscope of his own design. So, also, his second paper, on the spectrum of the Zodiacal Light, appears to have decided once for all a discussion which had occupied many observers.

"A series of five papers of great interest follow, on the gaseous contents of meteoric irons and stones. In the first of these he reviews the known results of the investigations upon the occluded gases of meteoric irons, quoting Professor Graham and Professor J. W. Mallett. In his own investigations, the material came, for the most part, from the collection in the possession of Yale University. His conclusions in this first paper were that no one of the several irons which he studied gave any spectroscopic evidences of unknown elements. The second paper is a

brief one upon the gases derived from the meteorite of February 12, 1875, presented as a note preliminary to a further study.

"In the following paper entitled 'Examination of gases from the meteorite of February 12, 1875,' Professor Wright gives a thorough review of the gaseous contents of this meteorite. It appears to be the first stony meteorite thus investigated and the results are of great importance; they not only show the presence of gases occluded in stony meteorites, but also prove that they are distinguished by having oxides of carbon as their characteristic gases, instead of hydrogen. He points out the bearing of these observations upon the peculiar spectra of comets and as a support of the meteoric theory of comets.

"In 1876 Professor Wright continued these important investigations, extending them to a considerable number of stony meteorites of known origin. The earlier conclusion that these stony meteorites are characterized by a large amount of occluded carbon compounds was abundantly verified, and the last paper contains a long discussion concerning the bearing of these observations on the current theory of comets. This terminates the series of papers on occluded gases in meteorites, but it is interesting to note that the mastery of the problems involved served him in an admirable piece of work five years later (1881). The paper—'On the gaseous substances contained in the smoky quartz of Branchville, Connecticut'—is sufficiently defined by its title, but the skill and success with which the investigation was carried out and the results presented make the article a model worthy of careful study.

"In 1877 Professor Wright published two important papers on the deposition of metallic films by the cathode discharge in exhausted tubes. A clear description of the technique of the process and of the physical properties of a large number of metals thus treated makes the papers of unusual interest. The intrinsic value of his method has proved so great that it is quite probable that the name of the author is more widely known from these scientific contributions than from any others published during his long and active life.*

*This important method of obtaining a metallic mirror was patented by another without his knowledge.

"In the foregoing review of the scientific work of Professor Wright there has been no effort to do more than sketch the contents of the papers of chief importance; a large number of notes and minor contributions to science have been passed over. It would hardly be just, however, to fail to note his activities in X-ray experiments. At a time when Röntgen's discovery was hardly more than a rumor and the greater number of physicists, perhaps somewhat skeptically, were awaiting more definite descriptions of methods and results, Professor Wright immediately applied the test of experiment and secured the first of these photographs made in this country. This showed in a very striking way his command of all the resources of his science at the time; nor did he stop with a mere verification of the most wonderful features of the phenomena. He made many studies of the nature of the radiations and their reactions on various forms of matter, but, like other contemporary investigations, the results were hardly more than negative and he published them only in a preliminary paper (1896) on the subject.

"This short review of the more important scientific work of Professor Wright, which was published and is attainable by everybody, does not by any means exhaust the list of his activities. His observations on the polarization of the solar corona made in Colorado on the occasion of the solar eclipse of July 29, 1878, were among the most important and conclusive up to that time. So, too, at a later date (1888) he aided the Government, as a member of a committee appointed by the National Academy of Sciences, in fixing the standards for polariscopic determinations of sugars at the national customhouses. This is interesting as having been one of the many occasions in which the Government has with profit turned to the National Academy for scientific advice. Another work of public character was performed in 1887, when he served as chairman of the committee of weighing on the Assay Commission to test the fineness of the gold and silver coins at the United States mint in Philadelphia. Further he was, from 1881 to 1886, an adviser of the Government Signal Service, but what he there accomplished has

not been specifically published, as far as known to the present writer.

"Of work, important, but of a different character from that already mentioned, were his labors on the two editions of Webster's Dictionary of 1864 and 1890. In the revision of 1864 he acted as general collaborator, assisting in the preparation of the manuscript for the press, the revision of definitions, especially those of scientific and technical subjects, and in reading the proofs. He also prepared for this edition the articles on 'Orthography' and the 'Rules' for spelling certain classes of words, and assisted in the revision of the list of 'Arbitrary Signs used in Writing and Printing.' In the edition of 1890 he gave his attention especially to the definitions in chemistry and physics. Professor Wright also prepared a series of summaries on 'Scientific Progress'."

Professor Wright's distinguished work in physics won for him wide recognition in scientific bodies. He was elected a member of the National Academy of Sciences in 1881; he was also made a Fellow of the Royal Astronomical Society of Great Britain in 1879; a member of the American Philosophical Society in 1896; of the Connecticut Academy of Sciences; the American Meteorological Society; the New York Academy of Sciences and other scientific bodies.

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1871

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1872

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The relation between structural and magneto-optic rotation (with D. A. Kreider); *ibid.*, vol. **6**, pp. 416-427.

1901

Spectrum of alternating current discharge studied with reference to its spectrum and especially the ultra-violet spectrum (with E. S. Downs); *ibid.*, vol. **12**, pp. 66-73.

In addition to his scientific papers, Wright also wrote a number of excellent biographical memoirs. Among these are to be mentioned:

Benjamin Silliman, Sr. (1779-1864), and his son, Benjamin Silliman, Jr. (1816-1885); these are published in volume 22 of the Ninth Edition of the Encyclopedia Britannica.

Alfred Marshall Mayer; *Amer. Jour. of Sci.*, vol. **4**, 1897.

Ogden Nicholas Rood; *ibid.*, vol. **15**, 1903.

Also for the National Academy of Sciences in 1911:

Benjamin Silliman, Sr. (1816-1885); and James Hammond Trumbull (1821-1897).

Another work of importance calling for much careful study is the "History of the Class of 1859 in Yale College. A Record of Fifty-Nine Years." This was prepared by him in 1914 as Class Secretary for the fifty-fifth reunion of the Class of 1859. As a matter of fact, no meeting was held, which gives all the more value to this volume of 288 pages.

It opens with an account of the semicentennial meeting in 1909 at which some twenty-four members were present. This was held at the house of Mr. Louis H. Bristol. Then follow the individual records of each member of the class, the graduates on pages 17-198, the nongraduates following on pages 199-248. The volume closes (pages 249-288) with a full and carefully prepared series of statistics of the class as to membership in the college societies, public honors, et cetera; also a list of the children and other similar matters of interest.

That the work involved much time and labor is at once obvious. Wright confesses this in the closing clause of his "Prefatory Note." This is worth quoting here entire since it shows also the rarely friendly character of the man. He says: "In conclusion, let me say that though the task of preparing the Record has been long and laborious, it has been most interesting, and the frequent communications to and from the members of the class a source of constant pleasure. If the result meets the approbation of my classmates it will bring great satisfaction to

"Yours most cordially, ARTHUR W. WRIGHT,
Class Secretary."

New Haven, September 1, 1914.

It is certainly remarkable that, at the age of seventy-eight years and only a year before his death, he was able to complete so large a work, and one calling for so much careful thought and labor.

ARTHUR WILLIAMS WRIGHT was certainly a rare man in character and in ability.

J. Walter Fewkes

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OF

JESSE WALTER FEWKES

1850-1930

BY

WALTER HOUGH

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1932

JESSE WALTER FEWKES
1850-1930

Jesse Walter Fewkes was born at Newton, Massachusetts, November 14, 1850, son of Jesse and Susan Emeline (Jewett) Fewkes. Both his parents were born at Ipswich, Massachusetts. His mother's ancestry traced to the close of the 17th century in America. The primary educational opportunities of the period were given the boy with the long view that he should have an advanced education.

The resources of the intellectual environment of Jesse Walter Fewkes at the period of 1850 were particularly rich in men and reasonably so as to material agencies. His family means, limited to the income of his father as a craftsman, were budgeted unalterably as to the item of education, reflecting the early American belief in this essential feature.

Thus the youth, Jesse, entered into the ways of learning through the primary schools and grades locally organized. There is no data giving a glimpse at his progress and capabilities in this formative period, only that a clergyman became interested in his education. Through this interest Fewkes was prepared for Harvard, and at 21 he entered the college without conditions.

His course in the school may be marked by two periods, in which he essayed to find his particular bent that would develop into a life work. In the branch of physics we find him leaning toward the advancing field of electrical science under that department at Harvard. The second and dominating branch of science in which Dr. Fewkes worked for many years was marine zoology. At the Agassiz school at Penikese Island, under the inspiration of the great teacher, he laid the foundation for his work on marine zoology.

Agassiz had the European attitude toward science and the art to give examples. This attitude considered learning as scarcely secondary to religion, a matter for intense propagation. It demanded a serious regimen, a devoted search for the truth, and the implanting of the same spirit in the consciousness of others. This explains the teacher Agassiz.

Pursuing this science Fewkes studied zoology at Leipzig, Germany, during the four years subsequent to his graduation at Harvard in 1875. As a student under the Harris fellowship he also spent a period of work in marine zoology at Villa Franca and Naples.

Returning to the United States, Dr. Fewkes was appointed assistant in the Museum of Comparative Zoology at Harvard. Here he spent nine years in charge of the lower invertebrates.

Dr. Fewkes' life work was divided into two periods of scientific pursuits, that of marine zoology and that of ethnology. The extent and seriousness of Dr. Fewkes' work in natural history may be seen in his extensive bibliography on marine animals, numbering 69 titles.

Dr. Fewkes' attainments as a marine biologist were of a high order and brought prompt recognition from students in all countries where this branch of science was pursued. He was chosen to write the articles Coelenterata, Discophora, Siphonophora, Ctenophora, and Actinozoa for the Kingsley Standard Natural History, Boston, 1885. Much of his time was given to the study of the Medusae. The phenomena of the formation of coral islands were investigated by him at a time when this subject began to attract the attention of scientific men. In the minute and painstaking work of describing the various forms of marine life he was proficient. His drawings of the anatomy of medusa and coelenterates attained the height of skill.

This second period is marked by the episode of Dr. Fewkes' entrance into the field of anthropology, which began on an excursion to the Pacific coast on the Santa Fe Railroad. Along the route in New Mexico and Arizona he saw numerous members of the peaceful Pueblo Indians who are not less attractive than their terraced communal villages.

The Southwest, a region ear-marked for exploration by Major J. W. Powell in 1868, presented a vast field for archeological and ethnological investigation, and had already claimed the work of Holmes, Stevenson, Mindeleff, Cushing, and others who may be acclaimed as the pioneers. The locating and surveying of archeological sites comprised the efforts of most of these early

workers. Mindeleff studied the architecture, ancient and modern, of the Pueblos, and Cushing confined himself to an intimate study in residence of the Zuni tribe.

At this period Mary Hemenway of Boston was interested in setting on foot researches in the Pueblo region, and Frank Hamilton Cushing was chosen to carry on the work. At the close of Mr. Cushing's brilliant studies, during which he suffered much in health, Dr. Fewkes, fresh from his travels in the west, was appointed leader of the Hemenway Expedition.

Cushing had confined his intensive investigations to the Zuni Indians of New Mexico. With these Indians also Fewkes took up the work of the Hemenway Expedition and published several papers on Zuni music and ceremonials. Later he transferred his headquarters to the Hopi Indians living on the isolated mesas of northeastern Arizona. The study of the Hopi comprised a good part of Fewkes' contributions to ethnology. His affection for the Hopi was profound.

From the beginning of his acquaintance with the Hopi Indians, Dr. Fewkes saw the importance of the record of the striking calendric ceremonies of this tribe. Evidently these ceremonies had come down from early times, and at the period of Fewkes' coming had been little modified. In this study Fewkes observed and recorded in detail, often at great sacrifice of personal comfort, all the rites of the ceremonial year, a body of scientific knowledge of inestimable value.

Interpretation of the archeological finds made during these explorations in the southwest was naturally a difficult problem. Confronted with this crux, Dr. Fewkes turned to the existing Pueblo Indians of the region to ascertain whether in the survivals of their social life there would be material to elucidate the subject. This lead proved most fertile. Due to the conservatism of religious practices Dr. Fewkes found abundant means of explaining the past by the present.

Particularly fortunate was the survival in isolation in the desert of the Hopi Indians, practically at the primitive stage at the beginning of Dr. Fewkes' researches. Study of the lore of these Indians gave Dr. Fewkes confidence in referring prob-

lems coming up in his explorations to the body of Hopi traditions and survivals of art and customs.

An example of the application of the natural history method is concerned with the artifacts resulting from the exploration of Sikyatki in 1895, during which hundreds of pieces of decorated pottery were excavated. Applying the interpretation of the art motives used in the decoration of ancient Hopi religious paraphernalia given him by the priests, Dr. Fewkes was able for the first time to fix the meaning of the Sikyatki symbolic designs. The sound indoctrination of Agassiz appears here. Gradually Fewkes' idea of the use of the Hopi complex as a source by which antiquity could be interpreted appealed to the common sense of scholars. It is felt that this major contribution of Dr. Fewkes will finally meet with general agreement.

RESEARCHES

Confining his work for several years to the Pueblo field, Dr. Fewkes on his entrance into the Bureau of American Ethnology broadened his inquiries to include regions that might yield data bearing on Pueblo culture. Important field work was undertaken on the Gulf coast of Mexico, the region of the first contact of the Spanish and containing possibilities of cultural contacts with tribes of the southern United States.

On his coming as ethnologist to the Bureau of American Ethnology in 1895, Fewkes continued his researches in the southwest. This was his most fruitful period, continuing to 1918, when he was appointed chief of the Bureau of American Ethnology. During the period of added administrative work he still found time to carry on field explorations. Even while visibly failing he completed a last work, the exploration of Elden Pueblo, an ancient site near Flagstaff, Arizona.

West Indian archeology and its relation, if any, to the cultures of the southern United States and to South America, respectively, was regarded by Dr. Fewkes as an important problem to be investigated. Dr. Fewkes' work in Porto Rico was published in a notable monograph regarded as a permanent con-

tribution to the subject. The West Indian researches were correlated with the work carried on by Dr. Fewkes in other islands of the group.

Returning to his favorite Pueblo field, Dr. Fewkes brought to a consummation his most valuable contribution to Pueblo archeology. Dr. Fewkes' idea was to contribute to the educational aspect of the ancient ruins by preparing them for study by carefully excavating the site and preserving and rendering them accessible to the public.

The traditions of the Hopi clans contained many allusions to the place of origin of the groups entering into the tribal complex. Dr. Fewkes found that there were clans from the north with animal totems, and clans from the south with nature totems, and clans from the east with totems of both classes. The traditions of the southern clans were more detailed and showed later data than from those of other quarters. Several years were spent by Dr. Fewkes in investigating the settlements of the clans named by informants. This quest led to explorations in the Little Colorado Valley along the river and south to the White Mountains, the ruins all showing pottery of the polychrome type and cream color, buff, and orange paste present. The study of clan origins was facilitated by the survival of a knowledge of the clan right to eagles in sites previously occupied by the Indians.

Continuing the work on the Little Colorado Valley, Dr. Fewkes penetrated to the upper Gila, a region marked by adobe villages, and also into the Red Rock country south of Flagstaff, where the Hopi still go for various needed products, such as roasted agave and bark for tanning or coloring buckskin.

In carrying out the work mentioned above of preserving ruins as assets of the educational system of the nation, Dr. Fewkes undertook the restoration and repair of Casa Grande in southern Arizona. This ruin, one of a number in the Salt River Valley, was seen as an extensive low mound of crumbled earth in which stands the remains of a massive adobe building known as Casa Grande, a landmark mentioned by early travelers in this region. Casa Grande was evidently the principal building

of an ancient city of low mud houses, the whole surrounded by a mud wall, forming a compound of great extent. Dr. Fewkes freed the base of this building from accumulated debris and placed over it a roof for protection against washing rains. A sufficient portion of the great compound was cleared to show the character of the architecture and arrangement of the cell units of the ancient city and the mud walls sealed over with cement. Through Dr. Fewkes' labors this monument is preserved for the enlightenment of thousands of visitors yearly.

Mesa Verde in Colorado is one of the most striking and valuable examples of Dr. Fewkes' idea of the preservation of ruins. Mesa Verde was the scene of the flowering of Cliff Dweller life. The remains here, both of open-air pueblos on the mesa and in the walls of the canyons, represent the Great Period of Pueblo culture. The region was high and inaccessible, but relic hunters before 1890 had done much damage. On the taking over of Mesa Verde by the Government as a national monument the National Park Service arranged with the Smithsonian Institution to have Dr. Fewkes carry on explorations and to preserve the ruins. This project took several years and gave a permanence and intelligibility to the Mesa Verde that is appreciated by the thousands who visit them yearly.

Among the many archeological fields in which Fewkes worked, the culture of the Mimbres Valley, New Mexico, proved to be important. Dr. Fewkes was pioneer in the investigation of the Mimbres' culture characterized by ceramics of unusual decoration in natural forms, described in several publications.

Summing up Fewkes' ethnological work among the Hopi it is seen that he provided a key by which could be interpreted the complex archeological remains of the ancient inhabitants of the southwest. Especially as to religious structures and symbolism is this applicable in the southwest.

By exploring type ruins in various parts of the southwest he contributed much of value to archeology. His methods, though not equivalent to the stricter regimen of present-day archeology, were adequate to the means and needs of the period. Especially in the collection and preservation of objects of material

culture he was preeminent. About sixty per cent of the material from the southwest now in the United States National Museum was collected by Fewkes. Important collections from the West Indies and Mexico were amassed by his efforts.

Another contribution was his method of preservation and repair of key ruins, an idea working toward economic and educational values. Still another contribution was his successful effort to induce the Government to set aside certain areas containing ruins as national monuments. Several of these reserves were established through his efforts.

Dr. Fewkes received many honors from learned societies and other bodies. He was made Knight of the Royal Order of Isabella la Catholica in recognition of his distinguished services at the Columbian Historical Exhibition at Madrid in 1892. From King Oscar of Sweden he received a gold medal "Literis et Artibus" for his discoveries in anthropology. He was a fellow of the American Academy of Arts and Sciences, member of the National Academy of Sciences, and officer or member of many scientific organizations. He received the degree of LL. D. for services in anthropology from the University of Arizona in 1915.

Dr. Fewkes was a likable, friendly man. Of above average height, well fleshed, sandy to reddish hair and beard, blue eyes, florid complexion, he was a personable man. He delighted to communicate his knowledge to all and common.

It was an experience of enduring memory to camp with Dr. Fewkes in the fascinating southwest. Under his leadership the glamor of the pursuit of the unknown filled the mind with pleasing pictures of new contributions to a young science. Each morning, after a night's sleep under the stars, everyone in the party returned vigorously to the work of excavation.

He married Florence George Eastman, who died in 1888, and again was married to Harriet Olivia Cutler, whose death preceded his by a few weeks.

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Thos A Edison.

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OF

THOMAS ALVA EDISON

1847-1931

BY

ARTHUR E. KENNELLY

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THOMAS ALVA EDISON

Birth and Parentage

Thomas Alva Edison, probably the greatest inventor that America has produced, was born at Milan, Ohio, on February 11th, 1847.

According to family records, the paternal ancestor of the Edisons landed in New Jersey, from Holland, about the year 1730. The family on Edison's mother's side, the Elliotts, was of Scotch-English origin and settled in New England prior to 1700. The Edisons were a vigorous, hardy stock. The inventor's great-grandfather, Thomas Edison, lived to be 104 years old, John Edison, his grandfather (1750-1852), to 102, and Samuel Edison, his father (1804-1896), to 92. They were all men of endurance and strong physique. Great-grandfather Thomas took an active part against the British Government in the Revolutionary War of 1775-1781; while grandfather John Edison, who was a young man of 25 when the war broke out, sided with the loyalists. When the great exodus occurred in 1783, and many thousands of loyalists embarked for Canada, John Edison was among them. Father Samuel Edison was born at Digby, Nova Scotia, in 1804. John finally settled at Vienna, Ontario, close to the northern shore of Lake Erie, and there he remained, as a Canadian citizen, to the end of his life.

In 1828, Samuel Edison, the inventor's father, married Nancy Elliott (1810-1871), a school teacher at Vienna. She was a clever, brilliant and attractive woman, the daughter of a Baptist clergyman and granddaughter of Captain Ebenezer Elliott of Scotch ancestry, who fought on the American side in the Revolutionary war, and who finally settled at Vienna, Ontario, where he lived to an age of over 100 years.

In 1837, when political disturbance in Ontario culminated in rebellion against the British government, Samuel Edison, 33 years of age, tall and of strong physique, was fighting as a Captain in Mackenzie's insurgents. For the second time, the younger member of the Edison family found himself on the

side of a lost cause. Samuel Edison had to make his escape with his wife, through dangerous country, to the southern shore of Lake Erie. They finally settled at Milan, Ohio. Here three children were born: William Pitt, Tannie, and the subsequently famous Thomas Alva, the last named on February 11th, 1847. All three children showed ability, but that of the two eldest was of an artistic and literary character.

Boyhood (1854-1868)

When young Edison was seven years old, his family removed from Milan, Ohio, to Port Huron, Michigan, through which town the Grand Trunk Railroad had laid its tracks along the St. Clair River which separated Michigan from Ontario. The boy was good-natured and courageous. Most boys are well endowed with curiosity, but little Thomas seems to have had curiosity insatiable. He used to express surprise that the grown-up people round him were unable to answer his numerous questions. He spent three months at the Port Huron public school, that being all the formal schooling he ever had. As a scholar, he does not appear to have made a success. His mind was keen enough; but did not follow the grooves of school learning. His mother, who had been a teacher, and understood him, attended to his education herself. Under her guidance he became proficient in reading and writing. Arithmetic he never cared for. He soon revealed a great thirst for experimenting, and especially for chemical experimenting. He spent his pocket money on inexpensive chemicals, and his spare time in the family cellar, trying out their properties—to see if what the book said was true. Throughout his life, Edison never accepted a text-book statement as final, until it had been tried out.

When the boy was eleven years old, he commenced his first venture in business, by taking family garden produce to market in Port Huron, in a little horse wagon, with the aid of another lad. At twelve years of age, he applied for and secured a concession from the Grand Trunk Railroad to sell newspapers on the trains between Port Huron and Detroit (a distance of 100

km. or 62 miles). In this way he was able to gain more pocket money for chemicals and experiments. He not only kept up this newsboy work on the trains; but also extended it, by employing other boys as assistants on other trains, and opening two small stores in Port Huron, each operated by a boy companion. Later he transferred part of his stock of chemicals from the family cellar to the baggage car of the train on which he worked. He was very popular with the trainmen, who knew him as "Al". A photograph of little Al, at about fourteen years of age, shows a cheery, plucky lad, in a train-boy's cap, full of keen interest in the world. About this time, he purchased in Detroit a small hand printing press, with forms and type. He persuaded the train conductor to let him mount this in the baggage car, and so organized not only the first railway chemical laboratory; but also the first railway printing press, collecting the news, composing, setting, printing and selling the sheets, all himself. The circulation of this single sheet, "The Weekly Herald", amounted to 800 copies.

It was in 1862, when Edison was fifteen years old, carrying regularly an eighteen-hour workday, that an accident occurred which left him with a permanent deafness in both ears. Running over a rough piece of track, the baggage car, with its Edison corner laboratory, jolted to the floor a stick of phosphorus. This started a fire in the car that took all the efforts of Edison and the train crew to subdue. The conductor was so angered that he soundly boxed young Thomas' ears. When the train stopped, all the stock of the laboratory and printing press was thrown out on the station platform, to his great distress. In Edison's opinion, the injury to his hearing, that may have been caused by the cuffing, was intensified later, when a friendly trainman tried to help him climb on a baggage car, by pulling on his ears.

The slight deafness which came on after these events became permanent and gradually increased in later years. A man of lesser calibre might well have become morose by this infirmity; but it certainly did not have that effect upon Edison. His disposition remained throughout life, sunny, kindly and serene.

In fact, he used to claim that his deafness was an asset; since it permitted him to concentrate his thoughts upon any desired object of study, even among intensely noisy surroundings. It was a remarkable vindication against fate, when this partially deaf man later discovered, invented and perfected the phonograph.

In August, 1862, an incident occurred which changed the direction of young Edison's career. The train on which he was newsboy was at Mount Clemens Junction, where freight cars were shifted. A box car was being shunted, at considerable speed, to a side track. The station agent's little son, the two-year-old Jimmie Mackenzie, had strayed to play on this side track, right in the way of the oncoming box car. Young Edison on the platform saw the danger. Casting aside cap and bundles, he jumped on the track and reached the child just in time to haul him clear. As it was, one front wheel of the car struck his heel, and threw him with the child to the side of the track, on the stone ballasting. Their faces and hands were cut; but no serious injury had been incurred.

On the following day, Mackenzie offered to teach the lad Morse telegraphy, with a view to helping him secure, later on, a position as railway telegraphist. Edison accepted, and in a few months, taking lessons three times a week, between train times, together with practice at odd hours elsewhere, he attained proficiency at the key.

For the next six years, Edison followed the career of a telegraphist. During that time, he wandered, in the service of the Western Union Telegraph Company, through a number of cities in the middle west, and the south as far as New Orleans. He became noted as a rapid and accurate operator, frequently being assigned to press work on night duty. He adopted for this work a clear, fast and upright style of handwriting, which afterwards always characterized his penmanship. He spent all his available leisure in experiment and study. Faraday's "Electrical Researches" particularly interested him, owing to their close dependence upon experiment, their imaginative appeal,

and their freedom from mathematical symbolism. He also acquired, in the course of his journalistic daily work, and with his retentive memory, a large fund of general information.

Entrance to Edison's Career as an Inventor (1868-1876)

In October, 1868, when Edison was 21 years old, he applied for his first American patent—a vote recorder. This was a device which enabled the affirmative and negative votes of a seated voting assembly to be swiftly recorded and automatically totaled at the chairman's desk. Edison succeeded, after much effort, in demonstrating the invention at Washington, before the appropriate committee of Congress, only to find that there was no demand for a mechanism of that kind. Unsuccessful but undismayed, Edison returned to his little workshop in Boston. About this time he gave up the career of a telegraphist, and devoted himself entirely to invention. He commenced with an improved telegraphic "stock ticker." In 1869, when he went to New York, the nation was off the gold standard, as a result of the recent civil war, and the market ratio of gold bullion to government notes was constantly shifting in the "Gold Room" of the Wall Street Exchange. A transmitting instrument, not of Edison's design, operated from a keyboard in the Gold Room, issued the fluctuating gold quotations over wires to brokers' offices in the vicinity, each office having a dial-indicating receiver. On the third morning after his arrival in New York, Edison was standing near the transmitter, which, as a specialist, he had carefully examined, when it became suddenly deranged by an internal accident, thus throwing out of action all the indicators in the connected brokers' offices. In the ensuing tumult, he realized the nature of the derangement and volunteered to correct it. His offer was accepted, and he was able to restore normal operation very speedily. This led to his being made manager of the system, which he proceeded to improve and develop with new inventions. The new Edison Stock Ticker was a great advance in many respects over the earlier device, and brought him much renown.

Edison used to describe, from personal observation of the gold-quotation system in New York City, the sensational events of the financial panic on Black Friday, September 24th, 1869, when gold went to a high premium. Prices rose so rapidly that his system had difficulty in keeping up with the market. It was characteristic of him, that while he was for a number of months an operating personage in a continual stream of speculation, he never speculated himself, nor even felt any impulse to speculate. He was constantly at work, either in the office or machine shop, testing or improving.

A few days after Black Friday, he entered into the first recorded American firm of Consulting Electrical Engineers, under the title of Pope, Edison and Company, 80 Broadway, New York.

After the successful sale of some of his inventions to the Western Union Telegraph Company, Edison opened machine shops at Newark, New Jersey, for invention and manufacture. He kept 50 workmen busy, and when orders came in heavily, a night force also. He served as foreman for both gangs, which meant living on the premises and taking short periods of sleep at odd intervals during the twenty-four hours. Here he developed a number of telegraph inventions, in particular, the quadruplex for sending and receiving four messages simultaneously over a single wire—two in each direction—, and the high-speed automatic telegraph. During the Newark period (1870-1876) he took out nearly 120 American patents, almost all in electric telegraphy.

In 1871, Edison married Mary G. Stillwell, by whom he had three children, Marion E., Thomas A., and William L. Edison.

Menlo Park Period (1876-1884)

Edison moved his laboratory from Newark in 1876, to Menlo Park, New Jersey, a small village on the Pennsylvania Railroad between Rahway and Metuchen, New Jersey, where he could concentrate on invention; since he found the combination of invention and manufacture too strenuous, even for his energetic temperament. This was the year of the Centennial

Exhibition at Philadelphia, at which the new Bell telephone was first shown to the public. The original Bell telephone apparatus did not have a satisfactory transmitter, and Edison, after much experimental labor at Menlo Park, produced a carbon-button transmitter that virtually converted the telephone from an experimental to a commercially available apparatus. It was in his experimenting with the carbon transmitter, that Edison coined the now well-known call word "Hello", which spread out from Menlo Park until it became adopted by telephonists all over the world. He also invented about this time, in Menlo Park, a new ingenious non-magnetic type of telephone receiver; since he was prevented from using the Bell electromagnetic receiver. It operated on the principle of electrolytically varied friction between two conducting surfaces in rubbing contact. This instrument, called the electromotograph receiver, had certain advantages in special cases, and produced very loud sounds; but did not come into extensive telephone use.

In 1877, Edison discovered and developed the first stages of his phonograph or talking machine, which was justly regarded as a great wonder at that time. Owing to the pressure of other inventions, however, he was reluctantly compelled to set this instrument aside for a time, and leave its further development to later years.

In 1878 he took up the inventive problem of "subdivision of the electric light". At that time the arc light, originally discovered by Davy in 1809, had been developed and had come into commercial use for the illumination of halls and streets; but it was too powerful for interior use, and the problem therefore was to substitute a number of small electric lamps for a single arc lamp. A number of inventors had already experimented with glow lamps or incandescent lamps; but no successful result had been achieved. Edison realized that a commercial incandescent lighting system would require its lamps to be connected "in parallel", between the main conductors, like the rungs of a ladder, and not "in series", or end-to-end connection, as had been the leading idea. This required that each lamp

should have a long, thin, high-resistance filament, a condition that greatly increased the difficulties of the problem. After a vast number of failures, his first partially successful lamp had a filament of carbonized cotton thread, mounted in a highly evacuated glass globe, with sealed-in platinum-wire leads. This lamp, in October, 1879, glowed for 45 hours before breaking. After that, success was gradually reached. It came through overcoming, in succession, a large number of small difficulties, any one of which could have destroyed the project.

A new industry had then to be created from the Menlo Park laboratory results. A lamp factory, a dynamo factory, factories for making operating instruments, testing instruments, main conductors, etc., all had to be set up and standardized. The early lamps were all operated by voltaic batteries, and although there were dynamos for operating arc lamps, there was none in existence for operating incandescent lamps in parallel. Such dynamos had to be designed and built. These were at first driven by leather belts from the standard low-speed steam engines of that period, the speed of which, although satisfactorily steady when driving factory machinery, fluctuated appreciably when used for incandescent lighting service. Edison realized that these belts could not be used in permanent reliable central stations; so new types of high-speed engines, with heavier fly-wheels, had to be developed for coupling directly to the dynamo shafts. After a number of small incandescent-lighting plants had been successfully set in operation, it was decided to open a central incandescent station in the center of the down-town business district of New York City, at 257 Pearl Street, with underground conductors. In all of this work, Edison was his own chief engineer.

The Pearl Street Station turned on its current to the lamps in the district, September 4th, 1882. It was successful from the first, the system gradually expanding over the whole of New York City. Although domestic gas lighting was then generally used, the new incandescent lamps won popularity through their steadiness, coolness, freedom from combustion products, and reduced fire hazard. About the same period, Edison incan-

descent-lighting stations and systems began to be developed all over the world.

Edison foresaw that the successful introduction of the incandescent lamp into factories and homes would immediately admit the use of the electric motor for operating machinery and household power devices. A few motors had already been employed on constant-current series-arc circuits; but the constant-speed self-regulating motor, taking power from constant-voltage mains, could not be produced until the incandescent lamp led the way. The first Edison motors were operated at Menlo Park in 1880, and rapidly developed on the Edison three-wire systems, and in Sprague electric-railway systems.

There were several standard characteristics of the Edison system worked out at Menlo Park, that have remained but little changed to this day. One of these was the lamp voltage, which he early set at or near 110 volts. A second standard was an inter-connected system of underground iron pipes, carrying within the insulated copper conductors which supplied the lamps. Edison realized that all these wires must go underground in large cities, and he faced the mechanical and electrical difficulties of that procedure, which were initially very great. The third characteristic was the invention in 1880 of an important division of the underground conductors into two classes: i. e., mains, which exclusively supplied the lamps, and feeders, which exclusively supplied the mains. By this means, a large saving was made in the total weight and cost of the copper conducting system. The fourth, very soon after the starting of the Pearl Street Station, was the invention of the "three-wire-system", by which, although the conductors in the system were increased throughout from two to three, yet the total amount of copper in the system was again reduced to about one-third; because all the conductors could be considerably reduced in size.

The demands of the electric light and power industry on Edison's time were so great that during his stay at the Menlo Park laboratory, he could give but little attention to the new discoveries and inventions which he was constantly making.

One of these discoveries was in 1883—the "Edison effect"; i. e., a discharge phenomenon that occurred in the lamps, when being exhausted; whereby a gaseous discharge passed from the glowing filament to the positive filament clamp, and also produced a deposit on the inner surface of the glass bulb. In later years, Fleming, De Forest and others developed this into the thermionic tube, now so widely used. Another discovery, which he made at Newark in 1875, was a device for generating and detecting high-frequency electric waves, such as Hertz investigated in 1889, and which later gave rise to modern radio telegraphy. Edison's generator was a vibrating-contact induction coil, or even an electric trembling bell, with one terminal connected to water-pipe ground. The receiver was a small wooden dark box, in which the opposed electrodes, consisting of graphite pencil points, could be brought into adjustable close proximity. When one electrode was connected to ground, and the other to any short open wire, small sparks could be seen to pass between the minutely separated carbon points in the dark box, if the latter was in the same building as the generator, or in some adjacent building. These results were so different from those ordinarily associated with electrical circuits, that Edison thought they might be some new phenomena, which he called "Etheric force". It required long researches of later years, in various countries, to link Edison's experiments of 1875 with electromagnetic waves and radio communication.

Llewellyn Park Period (1887-1931)

Edison moved his laboratory from Menlo Park to New York City, after his wife's death at Menlo Park, in August, 1884, and he gave up his home there soon afterwards. In 1886, he married Miss Mina Miller of Akron, Ohio. They made their home at Glenmont, Llewellyn Park, West Orange, New Jersey, and there they lived for the rest of his life. Their three children are Madeleine, Charles and Theodore. Edison built a commodious laboratory in West Orange, New Jersey, close to Llewellyn Park, and commenced work there in October, 1887.

At Orange, Edison perfected the phonograph, and made a long series of inventions, including the alkaline storage battery, the moving-picture camera, synthetic rubber, the telescribe, the magnetic ore separator, various improvements in manufacturing concrete and other chemical products, as well as many war inventions for the United States Government. The list of these Orange laboratory inventions is so long that it cannot be attempted here. For instance, he developed his moving picture camera until he obtained some satisfactory reels of sporting contests. The moving picture industry of today acknowledges its start at the Orange laboratory.

Personality

The outstanding qualities with which Edison impressed those who met him, were energy, frankness, courage and kindliness. He was tall, and powerfully built, with a large head and a countenance open and engaging, but leonine in repose.

He was a terrific worker, especially before his fiftieth year. He would often work at his laboratory or factory for twenty hours at a stretch, with only brief pauses for meals, concentrating on one study at a time. His assistants worked in the same way, and he did not spare them, because he never spared himself. It was not that he actually dismissed assistants who did not work hard; it was the easy-going assistant who dismissed himself and disappeared from the picture. When engaged on some difficult problem like the incandescent lamp, he and his staff worked together days, nights, Sundays and holidays, until they became unmindful of time. He was able to infuse them with his own enthusiasm.

When working regularly at a less feverish pace, he secured recreation from change of work. Above all things, he loved to invent some new or better thing than the thing he saw. He ordinarily carried about in his pocket a standard small-sized yellow-page notebook that might last a week. On the successive pages of this he would write down inventive ideas as they occurred to him, usually with some small illustrative sketch or sketches, together with the date and subject. He would stop

in the middle of a meal or conversation, or immediately on awaking, to write out an invention. In the course of a day, he might make twenty such rough designs. They might be nearly all in the same field; or they might differ greatly, depending much upon his surroundings during that period. One or two of these he would proceed to try out himself. The others he would probably distribute among his assistants to try out. He was ever incredulous about any invention that worked out successfully on the first trial, and always wanted to know what was the outstanding difficulty to be overcome. He was unfailing in encouragement and sympathy when difficulties beset an assistant's path, and suggested methods of surmounting them. Many of his first-sketch inventions proved to be impracticable, often for commercial reasons, and he did not expect more than a small percentage to survive laboratory tests; but fertility of imagination ran through them all. Moreover, he never sought to patent any process of which the inventive idea was not his own. More than one thousand American patents were issued to him during his career; but these were only for the residual inventions that he selected as probably workable and economically capable of self support. The vast majority never got beyond the notebook or the laboratory stages.

His method of attacking an inventive problem of major importance was always the same. He would attempt nothing until he had a clear comprehension of the existing state of the art, preferably from watching the latest process, or examining models; but otherwise by reading up the literature of the art. His memory for facts was most retentive, and he had acquired a habit of reading ordinary descriptive text by the line, instead of by the word, so that he could run through reports and pamphlets at great speed. He would then lay out in his notebook several plans for simultaneous experimental attack. As a confirmed optimist, he never doubted that an open path could be found for reaching the desired goal, provided that every possible plan was tried, regardless of established opinion or textbook authority.

His patience and tenacity in following up experimental improvements were most remarkable. He seemed to defy discouragement. He would spend weeks at a time on the improvement of the phonograph, teaching it, as he said, to say "specie". The delicate sybillant associated with the *c* was difficult for the instrument to render. It would return the word as "spee—ee". He succeeded finally to his satisfaction, but not without trying a very large number of devices.

In temperament he was simple, modest, direct and kindly. It was impossible for him to pose or assume airs. Although his practical knowledge was extensive in many branches of science, he never claimed to be more than an inventor. With the alertness of his active mind, he tended to take a definite opinion on any proposition that might be presented to him; but he would listen very tolerantly to opposite views, and give way pleasantly to facts or demonstration. In general, however. his views proved to be reliable and based upon experience. He possessed a certain charm of manner which endeared him to his associates. Furthermore, he had the rare gift of securing the mutual good will of his assistants; so that dissension among them was very exceptional. It is generally admitted that he won esteem and goodwill everywhere. His only detractors have been those who did not know him personally.

As an evidence of the loyalty of Edison's associates to him personally, it may be pointed out that, in 1918, a voluntary association was formed, called the "Edison Pioneers". The members are "Those persons who were associated with Thomas A. Edison, or connected with his work up to and including the year 1885." Associate members are those who came later (1886-1931, inclusive). The Edison Pioneers meet annually in New York City on Mr. Edison's birthday. At the present date (1932), there are 100 members and 136 associates. This memoir has been prepared with the help of the organization.

At public receptions, he was shy and retiring. He dreaded to be called upon to make a speech. In private life, however, he showed a remarkable talent for humorous narrative, and he

enjoyed listening to a good story. His life was a happy one. He was of very temperate habits, except in regard to hours of work. To the acquisition of wealth he was indifferent, except for the opportunities it brought for more inventions and accomplishments.

Honors and Awards

After his invention of the phonograph in 1877, honors came to Edison with increasing rapidity. The following honorary academic degrees were conferred upon him: Ph.D. Union College, 1878; D.Sc. Princeton, 1915; LL.D. University of the State of New York, 1916.

The following is a partial list of his medals and decorations:

Legion of Honor, France, Chevalier 1879, Officer 1881, Commander 1889.

American Institute of the City of New York, Medals of Superiority, for Electric Pen, 1878, for Multiplying Press, 1878. Medals of Excellence for Chemical Telephone, 1879. For Carbon Telephone, 1879.

Cross of Grand Officer (Count) of the Crown of Italy, 1889.

Rumford Gold Medal, American Academy of Arts & Sciences.

Albert Medal of the British Society of Arts, 1892.

Honorable Consulting Engineer, St. Louis Exposition, 1904.

John Fritz Gold Medal, 1908.

Rathenau Gold Medal (Germany), 1913.

Civic Forum Gold Medal, 1915.

Medal of the Franklin Institute, Philadelphia, 1915.

President of the Naval Consulting Board, 1915.

Distinguished Service Medal of the United States Navy Department, 1920.

First Recipient of Edison Medal of American Institute of Electrical Engineers, New York City, 1923.

Membership in National Academy of Sciences, Washington, D. C., April, 1927.

Society of Arts and Sciences Gold Medal, 1928.

Congressional Gold Medal, 1928.

Patents

The following is a list of 1,091 American Patents granted by the United States Patent Office to Edison during his career. They are listed according to the numbers whose applications were executed by him in each successive year between 1868 and 1928, inclusive, representing over sixty years of inventive activity. The individual titles of these applications up to the year 1926 are recorded in the Biography of Dyer, Martin and Meadowcroft (Bibliography No. 1). During the same working period he also had received up to 1910, 1,239 foreign patents, distributed among 34 foreign countries.

No. of patents granted	Date executed
1	1868
4	1869
7	1870
8	1871
38	1872
25	1873
15	1874
11	1875
12	1876
20	1877
14	1878
14	1879
60	1880
89	1881
107	1882
64	1883
24	1884
17	1885
29	1886
19	1887
45	1888
27	1889
36	1890
35	1891
5	1892
5	1893
0	1894
0	1895
1	1896
12	1897
9	1898
11	1899
22	1900
12	1901
20	1902
17	1903
18	1904
30	1905
18	1906
19	1907
24	1908
12	1909
20	1910
23	1911
19	1912
4	1913
6	1914
2	1915
5	1916
4	1917
1	1918
16	1919
4	1920
1	1921
3	1922
6	1923
7	1924
5	1925
5	1926
2	1927
2	1928
1091	Total

Chemical Manufactures

In addition to factories for producing dynamos, motors, incandescent lamps, etc., Edison designed and operated plants for the manufacture of cement, benzene, carbolic acid, myrbane aniline oil, aniline salt and paraphenylenediamine. A number of these plants were established temporarily for military purposes during the great war, but others are still in operation.

Termination

The long, industrious and internationally famous career of Edison terminated by his death at 8:24 world time (Greenwich Civil Time) on the 18th of October, 1931, in the 85th year of his age at his home in Glenmont, Orange, New Jersey. President Hoover, receiving the news on the battleship "Arkansas" in Chesapeake Bay, issued by radio a nation-wide commemorative address.

On the evening of October 21st, 1931, following his interment, being also the fiftieth anniversary of his effective invention of the successful incandescent lamp, at the suggestion of President Hoover, the Central Station system lights were switched off for one minute over extensive areas of the North American continent—the United States and Canada—as a nation-wide tribute to the great inventor.

Posthumous Honors

Shortly after Mr. Edison's death, a number of memorial meetings were held in different countries in honor of his life and accomplishments. At these meetings resolutions were recorded.

Thus, the Committee on "Production and Application of Light" of the American Institute of Electrical Engineers, in its annual report for 1932 (*Electrical Engineering*, June, 1932, pp. 447-453), concluded with the following statement:

"The Committee on Production and Application of Light cannot conclude its report without allusion to the passing of the founder of the electric lighting industry. His death on October

18, 1931, was the signal for an expression of world-wide appreciation of the services and of the life of the man whom electrical engineers have held in high regard by reason of his inauguration of electric light and power as a widespread service to the public. Edison passes but his work goes on. He lives in the esteem of those who carry on under the banner which he raised."

The Chekoslovakian National Committee of the International Electrotechnical Commission transmitted the following resolution to the Central Office in London:

"The Chekoslovakian Committee has the honor to present in the name of all the scientific and technical bodies in Chekoslovakia, the following proposition:

'That the name of the great American inventor Edison should be assigned to an electrotechnical unit.'

"The Chekoslovakian Committee hopes that this motion may be accepted by all the National Committees who desire to express their gratitude to the genius of T. A. Edison."

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T. C. Chamberlin

NATIONAL ACADEMY OF SCIENCES
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BIOGRAPHICAL MEMOIRS
VOLUME XV—ELEVENTH MEMOIR

BIOGRAPHICAL MEMOIR

OF

THOMAS CHROWDER CHAMBERLIN

1843-1928

BY

ROLLIN THOMAS CHAMBERLIN

PRESENTED TO THE ACADEMY AT THE AUTUMN MEETING, 1932

THOMAS CHROWDER CHAMBERLIN

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THE ANTECEDENTS

The Chamberlain family has been traced back to the ancient Norman counts of Sankville, knights from which stock took an active part in the conquest of England under William the Conqueror and were repaid for their services with large estates in the counties of Gloucester, Oxford, York, and Warwick. The founder of the family of Chamberlain in England, as an independent branch of the house of Sankville in Normandy, is said by some to have been Richard, "Grand Chamberlain" to King Stephen, who assumed his surname from his office.[1] According to another account, it was the Count of Sankville, chamberlain to King Henry the Second, who gave rise to the name in 1154.

In the early days of the American colonies several members of the Chamberlain family emigrated from England to the new country, among whom was Henry Chamberlain, who came to Hingham, Massachusetts, in 1638 from the parish of Hingham, County Norfolk, England, having crossed the Atlantic in the ship "Diligent," with his wife, his mother, and his two children. He was one of many who, under the leadership of the Rev. Robert Peck, fled from the religious persecution in England at that time. Later this branch of the family was engaged in ship building in New England. One member, Joseph, grandfather of Thomas Chrowder Chamberlin, went to North Carolina in the interest of the family's business, married Sarah Cartwright, and settled there. Ten of their children lived to maturity and evidently prospered, for the Chamberlains are said to have become the richest and most prominent family of Camden County, North Carolina. The second son, John, became convinced early that the system

[1] During the succeeding centuries over twenty variations in the spelling of the name came to be recognized in England.

of slavery, which played an important part in the plantation life of the time, was an evil one, and because of his strong feeling on the subject he left the slave-holding region for the newer country farther west. He traveled through Tennessee to Palestine, a small settlement on the Wabash River in Illinois. It was here that he met and married Cecilia Gill, a girl of Scotch descent who was born at Lexington in the Blue Grass region of Kentucky. Shortly after their marriage they moved about sixty miles westward. There, on the crest of the Shelbyville terminal moraine, a few miles southwest of the site of the present city of Mattoon, were born the three oldest boys—William Fletcher, John Nelson, and Thomas Chrowder. In this geologic setting, prophetic of his later interests, Thomas appeared on September 25, 1843.

THE BEGINNINGS

When Thomas was about three years old, the family started on a long journey to the wheat country of Wisconsin, traveling, as was the custom of the time, in two "prairie schooners," and camping many nights on the lonely prairie. Family tradition has it that the boy learned his letters on the way. When they reached Beloit, a small settlement just over the Illinois-Wisconsin state line, they found land to their liking and located near a grove on a ridge about four miles northwest of the village. The ridge-top above the rolling prairie was a stimulating environment for the development of a naturalist, for myriads of the now extinct passenger pigeons and countless other birds, each year migrated northward and southward and were set forth sharply on the black stubble left by the fall and spring prairie fires. Often at night wolves came up to the picket fence that surrounded the log cabin and quarreled with the dog till a blast from the old flint-lock shotgun scared them away.

One of Thomas' earliest remembrances was that of his father telling of the "laying of the corner stone," a matter which impressed the boy deeply, though he had not the slightest notion of its meaning. It referred to the founding of Beloit College, in 1847, an institution that had a profound influence on the boy in later years. When he was a small boy of eight a rather prophetic

incident occurred. One rainy day when Thomas and his two older brothers were playing together, one of them asked in fun what Thomas was going to do when he grew up. Thomas came back quickly with the reply: "I'm going to school till I can teach the best school in the state," at which the brothers laughed until Thomas became embarrassed. His father caught the answer and said, " Boy, don't you back out. Stick to it!" And Thomas stuck to it.

From the original log cabin the Chamberlin family progressed to a more modern farm house which they built about a mile away. When as a helper to the two older boys, Tommy quarried rock from an outcrop of Trenton limestone he debated with his brothers how the great "snakes" (portions of orthoceratites) could have gotten into the layers of rock and turned to stone. It was easy to see how they got down the crevices, but how could they get in between the layers where the boys found them? So, too, in digging sand for mortar, they found clay pebbles that were soft and could be broken with the fingers. They easily decided that these clay masses were young growing stones that had not yet hardened. This was the first school of geology the boy attended, and his only textbook was Nature's own, written a long time before.

In the course of time, two younger sons, Joseph Hanson and James Alexander, appeared in the Chamberlin family circle. The father, by preference a Methodist minister, by necessity a farmer as well, was known as a "circuit rider" because of his practice of riding around to several different meeting places each Sunday to preach. In time, this overtaxed his strength and eventually undermined his health. Though of limited academic education, he was an incisive thinker and reader in theological-philosophical lines, strict in his views, and greatly given to argument on religious subjects. He was regarded as abler than any of his boys and was long remembered locally as a man of exceptionally clear speech and systematic ideas. Thus the sons grew up in an atmosphere of serious and sharp debate in theology and philosophy.

In succession the five Chamberlin boys, Fletcher, Nelson, Thomas, Joseph, and James, entered Beloit College. The cur-

riculum was wholly prescribed, modeled on the classical courses of the colleges of New England, and on that of Yale in particular. In large part the course was Latin, Greek, and mathematics, though containing some history, philosophy, and science. Thomas was most deeply interested in philosophy and mathematics. Geology he faced with the attitude of a skeptic, for it did not at all agree with his theological beliefs. He determined to investigate the subject in order to show that his theology was right and that the geologists were wrong. So thorough were his investigations that, instead of convincing the geologists of their errors, he convinced himself of the truth of their contentions. When, at the end of the term, he went to his professor to find out his standing in the class, the older man turned and, with a twinkle in his eye, said: "Well, Chamberlin, would you like to know who stood next?" With this encouragement, Thomas became even more interested than before in the subject of geology.

On graduating from Beloit College, in 1866, Thomas Chamberlin was engaged as principal of the Delavan (Wisconsin) High School in which he had been preceded by men of more than ordinary ability and success. The situation was, therefore, highly stimulating to his work. He soon came to feel that the course of study was too narrow and barren to give the young men and women what they ought to know as an equipment for life, particularly in respect to the earth, and the heavens, and to the great events of the past. So he introduced a series of lecturettes on elementary phases of geology, astronomy, and other natural sciences, and occasionally on fine afternoons he took his students on field trips to explore the wonders of the out of doors.

In 1867 Thomas married Alma Isabel Wilson of Beloit, whose gentle, cheerful nature was the basis for the happiest home life and who until her death in 1923 was a great inspiration to him. Many of his early notes and manuscripts are in her clear, beautiful penmanship.

Since his equipment for teaching the innovations which he was introducing was very meagre, Thomas felt keenly the need for a broader foundation in natural science. He also foresaw the coming wave of scientific development. In 1868, therefore,

he and his young wife went to Ann Arbor for graduate study in science at the University of Michigan, which seemed to offer its learning for the least money. In 1869, after a year spent on a wide range of studies, including some work in geology under Alexander Winchell, young Chamberlin was called to the "settee" of the natural sciences in the State Normal School at Whitewater, Wisconsin. Though his field included all the usual sciences listed, by concentrating his study on one science after another during a period of two or three years, he became qualified to teach all of them to the extent required for his pupils.

It was here at Whitewater, while inspiring eager young minds with an uncommon enthusiasm for the phenomena of Nature, that he first found himself. In 1920, as a mature scientist, he wrote to his old pupil, Professor L. C. Wooster of the Kansas State Normal School, the following brief letter: "I still look back upon the beginning of my work at Whitewater as among the halcyon days of my life. To be sure, it was a stage of small attainments, but it was as full of endeavor to get on and get more that was worth while as any later days, and the fruits were just as sweet, for no fruits are perfect. They are merely pabulum on which to grow."

Of the results let one speak who came shortly after Professor Chamberlin left Whitewater.[2]

"When I first entered the school I found that it had a decided bent toward natural science. All the other departments were overshadowed by the predominance of this. The opinion of the teacher in natural science, even on Latin roots, was more highly esteemed than that of any other teacher in the school. Because of this prevalence of the scientific spirit, many a student received such an impulse toward the natural sciences that it developed into a passion, and gave direction to his future career. There has been a larger number of the earlier graduates of this school who have become teachers of science, or who have given unusual attention to some branch of it, than from any of the

[2] Prof. Lewis H. Clark, '79. Reminiscences of the Faculty. Historical Sketches of the First Quarter-Century of the State Normal School at Whitewater, Wisconsin, 1868-1893, p. 51. Madison, 1893.

other normal schools. I need but mention Professor King of Wisconsin University, Professor Salisbury of Chicago University, Professor Ewing of River Falls Normal School, Professors Culver and Buell of Beloit, Professor Wooster of Kansas, in evidence of this. I have thought that this was due to the fact that Professor T. C. Chamberlin was one of the earliest teachers of natural science in this school and made his department first in the esteem of the students."

THE GEOLOGICAL SURVEY OF WISCONSIN

In the spring of 1873, Governor C. C. Washburn signed a bill providing for a complete geological survey of Wisconsin and appointed T. A. Lapham chief geologist and Roland D. Irving, T. C. Chamberlin, and Moses Strong assistant geologists. To Chamberlin was assigned the southeast section of the state. The iron deposits lay in the central and northern sections, the lead and zinc deposits in the southwestern corner. The member of the legislature who, under Chamberlin's stimulus, had been most active in procuring the passage of the bill which established the Survey, on learning of the appointments of others to the mineral regions, said pointedly: "Mr. Chamberlin, you are shelved. What is there to be found in southeastern Wisconsin?" In reality, there seemed to be little more than the well-known early Paleozoic beds, covered up deeply with glacial drift. The apparently unpromising area practically forced Chamberlin to become a student of glaciation, and it was his studies in this field that eventually led him into the fundamental problems of geology and cosmogony.

From 1876 to the completion of the Survey in 1882, Chamberlin was chief geologist, instructed by the legislature to perform a task the comprehensiveness and difficulty of which may be gathered from the words of the enactment (Chapter 121 of the Laws of 1876):

"The people of the State of Wisconsin, represented in Senate and Assembly, do enact as follows: Section 1. That in the preparation of his final report, the chief geologist be, and he is hereby, authorized to collate the general geology and the leading facts and principles relating to the material resources of the

State, together with practical suggestions as to the methods of detecting and utilizing the same, so as to constitute the material for a volume suited to the wants of explorers, miners, land owners and manufacturers, who use crude native products, and to the needs of the schools of the State, and the masses of intelligent people who are not familiar with the principles of geology; said volume to be written in clear, plain language, with explanations of technical terms, and to be properly illustrated with maps and diagrams, and to be so arranged as to constitute a key to the more perfect understanding of the whole report."

The Survey, and particularly the chief geologist, had a hard task to perform. At that time the physical difficulties alone of so comprehensive a survey were enormous; the time allotted short; the legislature had to be cultivated for appropriations, and a capable group of collaborators had to be assembled and directed. Then, as always throughout his career, Chamberlin gathered around him able associates, among whom were Irving, Strong, Wright, Pumpelly, Sweet, Brooks, Whitefield, Wight, Wooster, King, Van Hise, and Salisbury. It was a time of great activity for the State Geologist, yet in the midst of these labors he served also part of the time as professor in Beloit College, and gave short courses of lectures on the relations of science and religion to audiences which filled the Second Congregational Church of Beloit.

That the Survey was strikingly successful is evidenced by the four grand volumes, "Geology of Wisconsin," in the last of which to appear (Volume I, 1883) the State Geologist carried out in remarkable fashion the difficult requirements imposed by the legislature. These volumes even today are highly prized by the people of Wisconsin and, as models of pioneer geologic research and of clear unfolding of the geology of a state, still stand without a peer.[3]

Chamberlin's scientific contributions ranged widely through many fields, but none was more characteristic of the man than the unraveling of the origin of the glacial deposits, the lead and zinc ores, and the Silurian coral reefs. Though "the survey in

[3] C. K. Leith, Chamberlin's Work in Wisconsin. Jour. of Geol., Vol. XXXVII (1929), p. 291.

the seventies swept over the Pleistocene formations and those that underlie them at the rate of about 4,000 square miles a year,"[4] Chamberlin's early studies of the Wisconsin Kettle Moraine were so outstanding as to win speedy recognition by geologists. In 1878, he went to Europe to present his results at the International Geological Congress in Paris and to study the workings of existing glaciers in the Alps. Upon returning to Wisconsin, he soon came to recognize that the prominent kettle moraine crossing the state in louped festoons divided an area of later glaciation from that of an earlier one. He thus recognized that the problem was more complex than he had supposed and that it reached beyond the scope of a single state survey.

Soon Chamberlin's Wisconsin glacial studies merged into the broader investigations which he started when he was appointed Chief of the new Glacial Division of the U. S. Geological Survey. Glacial studies at that time were somewhat further advanced in Europe than on this side of the Atlantic, but the penetrating examination of the unsurpassed glacial records left in the Upper Mississippi Valley soon placed American glacial investigations on strong, independent foundations and led to the important later developments.

Who can read the 200 pages in Volume IV, Geology of Wisconsin, upon the lead and zinc ores of the state, without realizing that here is a remarkable piece of work? The technical descriptions are rich in their fullness of significant details, and clear, lucid, and literary in diction, but the competent reader is most deeply impressed by the fertility of Chamberlin in framing and evaluating the possible hypotheses for the origin of the deposits. The "method of the multiple working hypotheses" is already being used. Hypothesis after hypothesis is tested with thoroughness and rejected because of its inconsistency with important facts. The favored view is discussed fully, carrying along several alternative sub-hypotheses, some one of which, or

[4] T. C. C.: Preface to the "Quaternary Geology of Southeastern Wisconsin," by W. C. Alden, U. S. Geological Survey Professional Paper 106 (1918), p. 13.

some combination of which, seems likely to loom large in the ultimate explanation when that stage shall be reached. The conclusions have been called daring in conception, and so they may seem, but they were the result of a far-seeing vision guided by rigorous logic and a mastery of the facts and principles involved. They have seemed daring largely because transcending the ordinary.

Chamberlin believed that the lead and zinc salts were carried to their present position in solution in descending meteoric waters which had obtained them from a more disseminated distribution in the Ordovician strata of the region. A zone of oxidation lay near the land surface. Below was a zone of "sulphuration" in close relation to shales so rich in bituminous matter that a fragment can be ignited with a match. Here occurred precipitation of the sulphides. Referring to the important percolating waters, he wrote: "That from the zone of oxidation carries in the metallic salts in solution while, below, there enter sulphureted waters and the two, mingling, deposit the ore."[5]

That some experts in ore deposits, coming later, attributed these ores to ascending hot solutions, worried him little, for he had great confidence in the essential correctness of his early work and he explained in a decisive way why the later interpretations seemed to him untenable. In fact, he himself had already discussed these alternative possibilities with great pains and rejected them in his original report.

In Volume II the peculiar dome-like structures in the Niagara limestone at Racine, Wauwatosa, Waukesha, and several other places in southeastern Wisconsin were minutely described and boldly interpreted as ancient coral reefs.[6] Fifty years later, Cummings and Shrock paid the following tribute to this truly surprising work: "Wisconsin has the honor of being the State in which the pre-eminent Chamberlin first accurately described ancient reef structures in America. His diagrams and descriptions (1877) leave little to be desired, and he correctly distin-

[5] Geology of Wisconsin, Vol. IV, p. 550.

[6] Geology of Wisconsin, Vol. II (1877), pp. 351-371.

guished both the reef and inter-reef lithologic facies and distinct faunas."[7] This remarkable diagnosis was several decades ahead of its time. Many other geologists throughout the whole span of years since 1877 have seen much of these prominent structures in Wisconsin, Illinois, Iowa, and Indiana; but lacking the daring imagination necessary for correctly explaining the various phenomena on exhibition, they have remained either baffled or incredulous of the reef interpretation.

Chamberlin made many more important contributions than that of describing and diagnosing the Niagaran coral reefs, but no single achievement seems more amazing than this early advance into the unknown. The training of the young State Geologist had been limited; he had traveled but little and at that time (1877) had never seen the ocean. Moreover, these quaquaversal structures were to him but minor features in a rapid survey of a large state. Only the true instinct of superior genius could have enabled him in those days to interpret correctly phenomena which have continued to puzzle geologists almost to the present time. Prophetic vision was the most outstanding characteristic of his mind, as was to be shown over and over again in later years.

ON THE UNITED STATES GEOLOGICAL SURVEY

In 1879, the United States Geological Survey was established with Clarence King as director, but the following year the directorship passed to Major J. W. Powell who reorganized the Survey into divisions. The success of the Wisconsin Survey, and particularly the character of its reports, had so strongly impressed Powell that he appointed Chamberlin to take charge of the glacial division of the Federal Survey. In transmitting this appointment on June 17, 1881, Powell gave the following instructions:

"The terminal moraine that enters the United States on the north border of the Territory of Dakota and stretches thence

[7] E. R. Cummings and R. R. Shrock: Niagaran Coral Reefs of Indiana and Adjacent States and Their Stratigraphic Relations. Bull. Geol. Soc. Amer., Vol. 39 (1928), pp. 579-620.

southward and eastward in sinuous course probably to the Atlantic will be the subject of your investigations for the coming year. You will also study such collateral deposits as may be intimately associated with its history. Your investigation should be devoted principally to that portion lying in the Territory of Dakota and to such other portions as have not hitherto been traced; giving to still other portions the examination that may be necessary to determine the relations of the several parts and the proper delineation of the whole on the map."

Thus commenced Chamberlin's work as geologist in charge of the glacial division of the United States Geological Survey, which continued until 1904 concurrently with many other duties in the later years. Carrying out the original instructions from Major Powell, Chamberlin worked from Montana through to the Atlantic coast. That there was an old glacial drift and a younger glacial drift, which in general did not reach so far south as the deposits of the earlier glaciation, he was quick to recognize, and he made it his task to trace the border of the younger drift across this great stretch of country. Others became associated in this work: J. E. Todd, in Dakota and Iowa; Warren Upham, in Minnesota; L. C. Wooster, in Michigan; and R. D. Salisbury, in New York. Earlier work in Pennsylvania by H. C. Lewis and G. F. Wright; in New Jersey, by G. H. Cook and J. C. Smock; and on Long Island, by W. W. Mather also aided much in the project. But the geologist in charge personally worked out long stretches of the chain, went over other links with his associates, and coordinated the whole into a comprehensive, well-considered report entitled "Preliminary Paper on the Terminal Moraine of the Second Glacial Epoch," which appeared in 1883 in the Third Annual Report of the United States Geological Survey.

Though ever scrupulously careful to give full credit to the work of others, it is apparent that the published report is very largely based on the observations and interpretations of the geologist in charge. Yet so rapidly were these studies completed that the finished report of 107 quarto pages was submitted by the director of the Survey to the Secretary of the Interior as a

part of the Third Annual Report on July 1, 1882, scacely a year after the assignment of Chamberlin to the task. The several maps cover the entire glaciated portion of the United States east of Montana, delineating the outer limit of the old drift, the limiting terminal moraine of the young (Wisconsin) ice advance, together with the directions of ice movement within the area of the young drift. Many refinements have since been made in the mapping and we now know that there were four main drift sheets instead of two, but "the importance of this paper was very great at the time of its publication."[8]

The Fifth Annual Report (1883-84) contained one of the classics of geological literature, "The Requisite and Qualifying Conditions of Artesian Wells," by T. C. Chamberlin. Artesian wells had been explained by French and English geologists many years earlier, but there had been no comprehensive analysis of the principles involved to compare with this for practical value. It was one of those publications of broad, fundamental principles and wide application for which the earlier years of the United States Geological Survey were notable and which built up the prestige that the Survey came to enjoy.

The Sixth Annual Report (1884-85) includes a joint paper by T. C. Chamberlin and R. D. Salisbury entitled "The Driftless Area of the Upper Mississippi." Within its 118 pages the reader finds a clear delineation of the characteristic features of the driftless area, compared and contrasted with the dominant features and peculiarities of the adjacent glaciated tracts. There is also an incisive treatment of the residual products of rock weathering characteristic of this area free from glacial scouring. A masterful discussion of the loess and its problems will also impress the reader; but what will strike him most is, no doubt, the thorough-going philosophical handling of the detailed problems, large and small, which combine to make the full story of the driftless area. Many of the conclusions were frankly stated to be tentative, some questions were left open for further development of knowledge, and for some of them the authors later

[8] W. C. Alden: Thomas Chrowder Chamberlin's Contributions to Glacial Geology. Jour. of Geol., Vol. XXXVII (1929) p. 299.

modified their views; but as a study in the methods of scientific investigation where the problems are complex, this report will make stimulating reading for a long time to come.

At that time Baron von Richthofen's eolian hypothesis of the origin of the loess was contending with the more generally accepted theory of aqueous origin. In the Driftless Area report each hypothesis was confronted with the facts presented by the loess of the Mississippi Valley, and the strong points and weaknesses of each were critically discussed. The aqueous hypothesis met a great difficulty in accounting for the widespread waters required by the distribution of the loess. In their thorough canvass of the possibilities, the authors considered ice dams which might have ponded waters over the loess region; and they enlisted the services of R. S. Woodward to consider mathematically the possibility that the attraction of the ice sheets by tilting the water level changed the drainage so as to furnish the requisite conditions for loess deposition in spite of the lowering of sea level by abstraction of water to furnish the ice of the continental glaciers. They considered also the possibility of crustal deformation due to weighting by the ice burden, and other causes. In the end they came very near to, but did not quite reach, the complete interpretation of the loess of this region as glaciofluvial silts swept up by winds from floodplains of streams from the melting ice and deposited widely. This theory, as set forth by the senior author in 1900, has hardly been improved upon by thirty years of subsequent study.

The assigned reasons for the driftless area in this location stand today just as they were outlined in the "Sixth Annual Report." To one familiar with the peculiar situation of the driftless area between tongues from the Labradorean and Keewatin ice sheets and with the well-known "Jardin," in the Talèfre basin of the French Alps near Chamonix, the poetical finishing sentence of the report is singularly full of meaning. "Diverted by highlands, led away by valleys, consumed by wastage where weak, self-perpetuated where strong, the fingers of the mer de glace closed around the ancient Jardin of the Upper Mississippi Valley, but failed to close upon it."

"The Rock-Scorings of the Great Ice Invasions" was a notable contribution which came out in the "Seventh Annual Report" (1888) after its author had become President of the University of Wisconsin. In an exhaustive study of the phenomena of striation, plucking, grooving, fluting, chatter marks, crescentic gouges and polishing, the records left on rock surfaces by glaciers were described and interpreted, and the inferences deducible from them as to the nature of glacier movement and its effects on underlying surfaces were developed in detail. Today, perhaps, we may be inclined to regard these features as of minor importance; certainly they were very simple compared with the profound philosophical studies into which the author was led in his later years. Yet his powers of observation and analysis and his capacity for taking pains are here revealed just as truly in this handling of small problems as in anything which he ever did. The accompanying glacial map of the United States, portraying the area of the earlier drift and that of the later drift, with the directions of striae for the eastern half of the country, records faithfully the state of knowledge in 1886.

The period of Chamberlin's greatest activity on the Survey closed in 1887 when he went to Madison to take up executive work as President of the University of Wisconsin. The heavy administrative burdens at Madison, and later the increasing diversity of interests at Chicago, left much less time for active field work, but by means of frequent conferences he continued to direct the work of the glacial division. He had trained several of his colleagues in these investigations, giving them the working criteria which they carried out with fidelity and painstaking care under his leadership. Thus it is, perhaps, only fair to state that much of the constructive synthetic thought and general interpretations which appeared in the glacial publications of the United States Geological Survey up to his retirement from general charge of the division in 1904, and for several years thereafter, were due directly or indirectly to T. C. Chamberlin.

PRESIDENT OF THE UNIVERSITY OF WISCONSIN

In the spring of 1885 Chamberlin had been offered tentatively the presidency of the University of Wisconsin, but he preferred to continue in his chosen field of geology. A second approach met with no more favorable response. The Board of Regents of the University, however, continued to press him, urging that the State of Wisconsin had given him his start and that his duty to the state lay in accepting the presidency. This to him was indeed an appeal hard to resist, and he was formally elected to the office by the Board of Regents on June 23, 1886. A full year, however, was granted for completing as much as possible of the Survey work.

When Chamberlin became President of the University of Wisconsin, in the early fall of 1887, it was still essentially a college. President Bascom had been a remarkable teacher and had brought from Williams College much of the spirit and tradition of a New England college. But a new type of educational institution was then coming into prominence with Johns Hopkins and Cornell as conspicuous leaders. The new president was young, vigorous, full of ideas, to a certain extent self-made and unhampered by tradition, and quick to see the best in new developments. The situation at Wisconsin was then favorable for educational advancement. New buildings to take care of the more pressing needs had recently been provided. Various other desirable but less urgent improvements could wait. Promptly and boldly he seized the opportunity to initiate a radical reorganization of the university.

So well has the nature of this reorganization been presented by Pyre in his excellent book on the University of Wisconsin that one can hardly do better than to quote extensively from it:

" 'The University of Wisconsin is in that transitional period in which it is easy to go either backward or forward,' wrote John Bascom, in his farewell *Report* to the Board of Regents. There was no doubt of the direction which President Chamberlin chose to pursue. With clearly defined purpose, he set about advancing the institution to a university basis. More distinctly than any predecessor he understood the university in three dimen-

sions. He saw at once the opportunity to extend it both in area and in depth. More distinctly than any predecessor, also, he grasped at once the triple function of the university, that of undergraduate and professional instruction, that of original investigation combined with graduate teaching, and that of "university extension," or popular dissemination of the essentials of advanced knowledge. Of these, the distinctively university function, he saw, was that of individual research and leadership in original study, and his dominant interest was in the development of the institution in this plane. But before this could be accomplished in any notable degree, the spirit of investigation must be conveyed into the undergraduate courses so that they might lead up naturally, not in information only, but in method, to work of the distinctive university type. In lateral extension of the curriculum, therefore, favor was shown to subjects of study which lent themselves to modern methods of investigation and teaching; or which, as in the technical departments, were most forward in the application of science to industry. Of several departments established at this time, it could be said, and was, that their counterparts had not previously existed in any university. In order to facilitate all of these activities it was desirable that the university should be organized on a more logical and workable plan."[9]

At that time the Agricultural College was an experimental station with an occasional student. The Engineering School had departments, and the last years of the engineering work were separated from the general scientific work of the college. On the other hand, the College of Letters and the College of Arts overlapped. The nominal divisions of the University did not coincide with the actual alignment of interests. Quoting again from Pyre:

"Accordingly an act of legislature was procured in 1889, whereby the old organization was abolished and the University was reconstituted into the four colleges of Letters and Science, of Agriculture, of Engineering, and of Law. All the departments of pure knowledge and investigation, including the physical sciences. were associated together in a single college, with complete autonomy as to its internal affairs; each of the more powerful divisions of professional or applied knowledge was erected into an independent organization with which the central

[9] J. F. A. Pyre: Wisconsin (1920) pp. 258-259.

college might have such relations as should seem, at any time, reciprocally advantageous."[10]

Thus commenced the system of organization now in operation at the State University.

"The development of the special colleges constituted one phase, and an important one, of the transition from college to university. Equally fundamental were the changes wrought in the central College of Letters and Science. Here again the University is indebted to the initiative of President Chamberlin. Had he, in his sympathy with natural science, elected to give the institution a powerful impetus in this direction, it could not have been considered remarkable. Nearly half a million dollars had just been expended by the State for the new group of science buildings and the nucleus of a strong scientific faculty had already been collected. The opportunity for a special development was obvious. Nothing in Chamberlin's administration, therefore, is more noteworthy than the largeness of spirit in which he set about to strengthen the humanities. But he saw with great clearness that, if the humanities were to be maintained in competition with science, they must be reanimated by an infusion of as much of the modern spirit as was compatible with their nature. 'The remarkable advance which the natural sciences have made in recent years as educational factors,' he wrote in his earliest *Report,* 'has been dependent very largely upon the laboratory and field methods which have given them vitality and effectiveness. Parallel methods in other departments of study undoubtedly mark a coming era of vigorous growth and commanding influence.' "[11]

"For the purpose of permitting greater concentration, continuity, and thoroughness in the leading lines of study"[12] the Group System was installed, whereby the first two years of the undergraduate work were devoted to the acquisition of a general education through a basal group of required subjects, to be followed in the junior and senior years by a "major" line of study. The latter was introduced not as a requirement, but as optional. The student was free to follow what was called the Course System, which was substantially what the University had, or he could follow the Group System in which he would con-

[10] Pyre: op. cit. p. 259.

[11] Pyre: op. cit. pp. 280-281.

[12] Pyre: op. cit. p. 282.

centrate his work more and more in the last two years of the course. The announced purpose of this system was "to introduce university methods, in the modern sense of the term, more largely in the undergraduate college course, and so prepare the way for the better development of graduate work." [13]

"Then he hoped to concentrate the informational aspects of education, the part of education which consists in accumulating a reasonable amount of handy information that belongs to a college man, by a system of short lecture courses known as Synoptic Lectures. It was a fine idea, and about the only phase of his work that has never been adequately developed. If he had stayed here, he would probably have developed that, but it took a genius and a great deal of energy to develop an idea so novel. The modern form of it appears in numerous orientation and adjustment courses that we see in colleges, none of which looks as attractive in many ways as Chamberlin's original ideas." [14]

"The introduction of the Group System necessarily had two effects. One was that it brought about the introduction of a considerable number of elective studies for the advanced undergraduate students, and the other that it led inevitably to the continuing of this advanced work as graduate study; so that the next thing which Chamberlin did was to begin the formal development of graduate study. He did that in three ways; in the first place, by encouraging the offering of courses; in the second place he established graduate fellowships for the first time in the history of the University; and in the last year of his administration he organized the School of Economics, Political Science, and History, and brought Dr. Richard T. Ely from Johns Hopkins here to head it; and that was very definitely to have graduate study as the primary object. Of course this movement toward graduate work antedated the founding of the University of Chicago, and I do not believe that any university in the Middle West was moving specifically toward graduate study in the way that we were during the early part of Chamberlin's administration." [15]

He found a strong faculty when he came, and he added greatly to it. Very early he called Joseph Jastrow from Johns Hopkins to undertake the establishment of a laboratory of psychology. At that time the only well-established psychological laboratory

[13] Pyre: op. cit. p. 283.

[14] Oral statement of C. S. Slichter to J. V. Nash, Dec. 18, 1928.

[15] Oral statement by E. A. Birge to J. V. Nash, Dec. 18, 1928.

was at Johns Hopkins, though the year before Cattell had started his work at the University of Pennsylvania. This innovation at Wisconsin was not only the first of its kind in the Middle West, but also the first connected with a state university. Other advances soon followed: "Wisconsin was the first state in America to give a short course in Agriculture in her university. It had for its purpose the calling of young men from the farms to Madison during the winter months and training them so they could pursue a line of farming which would be more remunerative and on a much higher plane than that which then existed in the state." [16] It was largely through President Chamberlin that this course became a reality.

One of Chamberlin's early students at Whitewater Normal School had been Franklin King, who would have been dropped from the school for unsatisfactory work but for the insistence of his natural science teacher, who saw promise in him. Later at Madison he appointed King to a professorship in agricultural physics—the first chair of that kind in existence. Even to this day (long after his death) the work of King is recognized the world over. "When Sir John Russell, of the Rothamstead Agricultural Station in England, visited Wisconsin he put first the work in soil physics started by King." [17]

Building up the School of Engineering was a project into which Chamberlin threw himself with much enthusiasm. For this he gathered together a very eminent staff, only to lose several of them to Leland Stanford University just before he himself went to Chicago. President Jordan was said often to have remarked that he waited to see who Chamberlin called to his faculty and then took them away with higher salaries. The disappointment over losing the pick of his engineering staff was a factor in Chamberlin's accepting the Chicago post.

In his last year at Madison, Chamberlin launched another bold project. Recognizing that no university can be great in all lines of thought but should concentrate upon certain selected lines, he decided to emphasize economics, political science, and

[16] Ransome A. Moore: Hoards Dairyman. October 10, 1928, p. 876.
[17] C. S. Slichter: Statement to J. V. Nash. December 18, 1928.

history. Plans for a School of Economics, Political Science, and History were developed and approved by the Board of Regents, thus making it possible to give greater opportunities for graduate work and research within that unit than in the University as a whole. Because of the unusual opportunities offered for a powerful development of graduate work, he was able to secure as director of the new school, Dr. Richard T. Ely, head of the Department of Economics at Johns Hopkins University. Ely came to Wisconsin just as Chamberlin left for Chicago, but the new movement was already well under way and was destined before long to make Wisconsin great in economics, political science, and history.

"It was, however, characteristic of Chamberlin that he never 'played his work up' so as to make his plans seem greater than they really were. He did not minimize their importance but he refrained even from indicating the results which the future might bring out of them. In this case he did not talk of founding a graduate school; he knew that neither the State nor the University was ready for such a movement. But just as he began his work at Wisconsin by encouraging graduate study through fellowships and by the offering of graduate courses in several departments, so the significant step which came at the close of his administration was the founding of this School which should be more specifically devoted to graduate study.

"This developmental method marked all of his administration. Chamberlin very definitely united his forward movements to the past history and the present state of the University. He did not desire revolution either in fact or in form. He advanced the University along the lines of development which, as he saw, were those that belong to a midwestern state university and were equally those along which the University of Wisconsin was ready to move." [18]

At Chamberlin's final commencement in 1892, the University for the first time granted the degree of Doctor of Philosophy for graduate work pursued at the University, although the degree had been conferred, *honoris causa,* before. It is interesting to note that the first man to receive this degree was Charles R. Van Hise, later (1903-1918) President of the University.

[18] E. A. Birge: Statement to J. V. Nash.

In leading the institution along the path of progress from what was essentially a college into a university both in organization and in spirit, President Chamberlin had moved steadily forward in a series of successful achievements. His experience as State Geologist had given him a wide knowledge and understanding of legislatures. He had come to know that in dealing with legislatures everything is likely to go well at first, but that later the ship seems to scrape on the bottom. When that happened Chamberlin knew exactly what to do, rather by figuring it out than by instinct, for he did not have the instinct of a politician. Understanding the way in which things were likely to go, he was never surprised and did not have to improvise, which made him probably the University's most successful president in getting appropriation bills through the legislature. He knew likewise how to move in order to get measures through the faculty as well as through the Board of Regents.

For all of the larger sides of administration, according to Dr. Birge, Chamberlin was a very exceptional man. But the very qualities which insured success in these larger affairs were a hindrance to him in many small matters. He was likely to ponder over petty matters as though they were important scientific questions, whereas when a student wants to know whether he can be excused from class on Friday, so as to go home, he wants an answer, "yes" or "no," right away. Minor details piled up on Chamberlin and worried him and made the administrative work more of a burden to him than it should have been.

In 1891, when President William R. Harper was gathering together outstanding men for the faculty of the new University of Chicago, he chanced to learn that President Chamberlin might welcome a return to his scientific work, and he promptly offered Chamberlin the headship of the department of geology to be organized at Chicago. The opportunity seemed golden and Chamberlin felt that he had earned the right to accept it. In five years as president he had fulfilled his obligations and duty to the State of Wisconsin by reorganizing the University and preparing it for an even more rapid advance to follow. With a deep feeling of relief at being free from the burdens of the University presi-

dency, he turned hopefully toward Chicago. He had been "a pioneer in the educational field, who saw the possibilities which seemed visionary at the time, but which he fortunately lived long enough to see realized and justified. The remarkable development of the University of Wisconsin which he lived to see in other hands justified his early anticipations; and it was a particular source of gratification to him that a part of this program was realized through the activities of Van Hise, whose career in geology, as well as his administrative policies, was so largely influenced by the example of his master, Chamberlin." [19] The work had been well done. "No quinquennial in the history of the University has yielded results greater, more permanent, or more beneficent," wrote President Emeritus Birge.[20]

AT THE NEW UNIVERSITY

President Harper gathered his faculty for the new University of Chicago from the four quarters of the earth. Rarely, if ever, at the inauguration of an institution of learning, had there been brought together men of such varied academic experience and such diverse points of view. Chamberlin came because he welcomed the relief from the burdens of administrative work, but despite the fact that he had resigned the presidency of the University of Wisconsin, when it was in a flourishing state and in the face of a unanimous petition of the faculty and students that he should remain, in order to return to scientific research, he was unable to keep free from participation in the organization of the new University during its earlier years. He felt forced to accept for a time the deanship of a scientific section, the Directorship of the University Museums, and other duties in addition to the development of the Department of Geology. He became the acknowledged leader of those who urged greater freedom for the sciences and less requirements for the classics, though he favored the full development and free election of the latter.

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[19] Joseph Jastrow: Letter to J. V. Nash in 1928.

[20] Historical Sketches of Beloit College. Edward Dwight Eaton (1928), p. 253.

In those days Latin and Greek were the backbone of a general college course, and on them the student was required to focus much of his attention. President Harper's own scholarly achievements (phenomenally great for so young a man) had been in the field of the classics, and he had had little contact with the growing field of science. The University of Chicago started with what seemed to Chamberlin an overemphasis on the classics. He himself in preparatory school and college had studied Latin for eight years and Greek for five. Fully appreciating the classics, he was in no way opposed to their full development and the free election of these studies by those so inclined, but he did feel most strongly that, with the rapid extension of the domain of science, students entering the scientific fields were greatly hampered by excessive requirements of Latin and Greek. Thus started the memorable succession of debates in faculty meetings principally with Professors William Gardner Hale, of the Latin Department, and Paul Shorey, of the Greek Department, on "the Latin question." The faculty naturally was divided in sympathy, and the contest was long-continued and severe. Science gradually emerged victor in this struggle, and the vigorous efforts of Chamberlin paved the way for the great expansion of the University in scientific lines.

The University inaugurated the four-quarter system, instead of the usual two semesters, and at the close of the Winter Quarter in 1893 Chamberlin gave the University's second Convocation address. To add momentum to the movement which he was championing, he chose as his subject: "The Mission of the Scientific Spirit." Two paragraphs of this address which introduce the scientific spirit may be quoted:

"In the architecture of science, every beam is to be tested, every joint is to be put to trial. Conjectures, assertions, opinions, current impressions, preconceived notions, accepted doctrines, all alike are pushed aside to give free scope to untrammelled induction from carefully sifted evidence. The supreme endeavor is to present a disposition of fairness and openness to all evidence and all inductions. Whatever evidence demands, that it accepts. Whichever way the balance of evidence inclines, to that it leans. There is no resistance to the leadings of evidence, there is no

pressing of evidence to give it greater or less than its intrinsic weight. All lines of inquiry are pursued with equal zest. All phenomena are welcomed with equal cordiality. The mind opens itself on all sides to every avenue of truth with equal impartiality.

"When demonstrative realities are brought forth they are embraced to the exclusion of all else. They displace all preconceptions, all deductions from general postulates, all favorite theories. The dearest doctrines, the most fascinating hypotheses, the most cherished creations of the imagination or of the reason are cast aside that the new light may freely enter and illuminate the mind. Previous intellectual affections are crushed without hesitation and without remorse. Demonstrative facts are placed before reasonings and before ideals, even though the reasonings and the ideals seem, from previous bias, to be more beautiful, to be more lofty, yea, even though they should seem for the time, until the clearer vision comes, to be truer. That which at the first seems absurd, that which for the time seems impossible, still ofttimes proves to be true in the light of a rectified vision of real relations. And so, the scientific spirit prompts to the acceptance of duly determined facts however they may accord with preconceived standards."

Farther on we read:

"The most significant and prophetic fact in the history of material studies is the tendency to the dematerialization of matter. Starting with the common notion of the fixity, inertness, and passivity of matter, it was discovered, step by step, that activity was a more and more pervading and potential characteristic of all forms of matter from the atom to the falsely-fixed star. It was early discovered that some of the very elements of fixity were but expressions of motion. The gyroscope was a key that unlocked a wondrous treasure-house of hidden truth. As research advanced the functions attributable to fixed solid matter diminished, until the greatest living English-speaking physicist felt warranted in advancing a kinetic theory of matter; a theory which almost completely eliminates matter as ordinarily conceived, leaving only an ulterior, fluidal, ethereal entity—for I scarcely dare say matter—which has become a basal concept in modern ultra-physics. Vortices of intense, incessant, irrotational motion are conceived to be the real units of physical phenomena. Towards this view evidence and opinion seem tending. It is not demonstrative, but it seems a prophetic vision in the line of coming truth. And so it appears but a prolongation of intellectual perspective to anticipate the entire elimination of the current view of matter

as a true concept and the substitution therefor of inconceivably intense activity in a basal entity whose characters are just beginning to disclose themselves. Certain at least it seems that the future concept of matter must have as its central factor inexpressible intensity and refinement of activity. Certain it seems a picturing of the real constitution of matter must be antipodal to what we have inherited, must awaken higher sentiments and call forth the utmost resources of the true imagination."

This was in 1893: quite different was the view frequently expressed in the years immediately following, that "future advances in physics will be largely in the fifth decimal place."

In turn there were treated biologic investigations, historical inquiries, science and volition, the new humanities, philological research, literary development and religious phenomena, to bring home to the university community a realization that the scientific spirit is ideally and truly a vital factor in all advanced human thought.

"The curtains of the heavens have been folded up and laid away as the garments of our children; as things loved but outgrown. Olympus is gone. Milton's Cosmos equally with his chaos is only a picture of the past. The richest imagery of all past literature has lost its power save as a glory of the past. And this simply because it was not true. The heavens are not as they were imagined. The beauty of thought does not make it true. The loveliness of thought does not make it immortal. Only the true is enduring. We shall still love these literary myths as marvelous products of days and conditions that are gone. They rightly teach us as all past life productions teach the appreciative soul. Rightly viewed their value is even heightened by the very fact that their day is gone to return no more. The bone that lies in the gutter is a matter for the scavenger. The bone embedded in the Cambrian shales is beyond price. And so it is with the literature that marks the evolution of the thoughts and feelings of the ages. As products of the past their value is beyond estimate. As factors of present and future creations they have lost their potency."

With the beginning of 1893, three months after the University opened its doors to students, appeared the first number of the *Journal of Geology*. Its announced purpose was to serve as a medium of publication for the results of researches connected

with the department, to bring to the University and its constituency some of the products of leading geologists in this and other countries, and to be a means of intercommunication between investigators throughout the world. The breadth of plan at the outset may be gathered from the imposing list of associate editors from outside the University—Archibald Geikie, Hans Reusch, Charles Barrois, Albrecht Penck, Gerard DeGeer, G. M. Dawson, O. A. Derby, Joseph LeConte, G. K. Gilbert, H. S. Williams, J. C. Branner, G. H. Williams, and I. C. Russell. The opening article of Volume I was "On the Pre-Cambrian Rocks of the British Isles," by Sir Archibald Geikie. Followed by other noteworthy contributions, reviews and editorials, the initial number of the *Journal,* in spite of the scant time available amid the chaos of the University's first quarter, compares favorably with any later issue.

To keep the *Journal* at the high level of its beginning imposed a severe task on the editor in chief, particularly in the earlier years. Contributions of desirable manuscript sometimes fell short of filling the numbers and more material was needed. A series of essays under the heading of "Studies for Students" was accordingly inaugurated to fill gaps when required. To this series both Chamberlin and Salisbury contributed from time to time descriptive and expository studies which presented in model form the latest developments of knowledge and the best thought of the times along certain lines which they selected. Though modestly addressed to students, some of these "studies" proved to be of lasting value. In this unpretentious series appeared "The Method of Multiple Working Hypotheses." So steady has been the stream of requests for copies of this little essay during the thirty-four years since it first did its duty, that it has recently (1931) been reprinted in the *Journal of Geology* in its original form.

Later, with the growth of the science, there was no shortage of manuscript and Chamberlin's own productions were oft held back to give precedence to the work of others.

PEARY AUXILIARY EXPEDITION TO GREENLAND

Before Lieutenant Robert E. Peary left the United States in 1893 on his third trip to Greenland in quest of the North Pole, he made arrangements for an auxiliary expedition to join him the following summer and furnish his party with additional supplies should it remain for another year's work, or offer it a means of return should its work be finished. On July 7, 1894, the Peary Auxiliary Expedition, comprising Henry G. Bryant, commander; William Libbey, geographer; T. C. Chamberlin, geologist; Axel Ohlin, of Sweden, zoologist; Emil Diebitsch, civil engineer; H. L. Bridgman, historian; and H. E. Wetherill, surgeon; sailed from St. Johns, Newfoundland, in the *Falcon,* a small but staunch sealing steamer specially fitted for ice work.

Early on the morning of the 12th of July, Cape Desolation was sighted. The first glimpse of the topography of Greenland was full of suggestions. The coastal mountains were rugged in outline and rough and angular in detail. Flowing contours scarcely entered into the general view. The dominant lines of sculpture ran in vertical rather than horizontal courses, and led to the inference that they owe their fashioning to the familiar meteoric agencies that work vertically rather than to the horizontal rasping of ice flowing out from the Greenland ice cap. The bearing of this angular topography upon the problem of the former extension of the ice, and especially upon the broad question whether the ice sheet of Greenland once crossed the Baffin basin and became the source of the North American glaciation, as held by some geologists, at once presented itself and stimulated a careful scrutiny of the topography throughout the whole length of coast northward to Peary's headquarters on Inglefield Gulf.

As the *Falcon* steamed slowly northward it was found, however, that angularity was not a universal feature, but that it alternated with very considerable tracts of flowing outlines of glaciated aspect. In fact, about one-half of the western coast of Greenland between Cape Desolation and Inglefield Gulf, a stretch of some seventeen degrees of latitude, presented angular and apparently unglaciated contours, whereas the other half

presented subdued outlines indicating former glaciation. North of Cape York, Chamberlin found two islands of such significantly different configuration, though composed of almost identical gneissic rock, that they have since become the familiar textbook illustrations of a rugged conical island before and after an ice sheet has passed over it. Dalrymple Rock, near the Greenland coast about 77° north, is exceptionally jagged and rugose, with no signs of glacial abrasion either in its general configuration or in its surface details. Quite in contrast are the Carey Islands out in the middle of North Baffin Bay, thirty miles to the west northwest, which have been worn smooth in outline. Striae on their scoured surfaces show glacier movement from the north coming from Smith Sound rather than from the Greenland coast. On Southeastern Carey Island were also found erratics of limestone, sandstone, shale, and quartzite wholly unlike anything seen in the islands themselves or known to occur on the mainland of Greenland to the eastward, but such as might apparently have come from the sedimentary series of Grinnell Land and the extreme northwestern coast of Greenland. The natural pathway of the ice bearing these boulders would be through Kennedy Channel, Kane Basin, and Smith Sound. But that ice did not extend as far as Dalrymple Island near the Greenland shore.

Later a small driftless area was found near Peary's camp on Bowdoin Bay, indicating that the former glaciation in this region did not extend greatly beyond the limits of the present glaciers. From these and other lines of evidence Chamberlin came to the conclusion that, although the ice of Greenland formerly extended somewhat beyond its present limits, it did not have any great amplification during the glacial epoch, at least in a westerly direction. Greenland was not the source of the vast ice-sheets which spread over much of northern North America during Pleistocene times, as many had previously thought.

After many struggles with the Baffin Bay ice pack, at times following narrow lanes of open water amid the floe ice, at other times crashing full speed into the ice to split it asunder, the *Falcon,* on July 25, reached Murchison Sound, the northern entrance to Inglefield Gulf. Peary's headquarters were at the

head of Bowdoin Bay some thirty-five miles up the Gulf, but the ice pack was late in breaking up that summer and a week elapsed before the camp was finally reached.

In their attempt to cross the great ice cap to the northern end of Greenland, Lieutenant Peary's party had encountered a succession of terrific storms, which, together with the death of many of the dogs drawing the sleds, had forced a return to the base camp and the abandonment of the project for the year. Although most of his party were greatly discouraged, Peary himself was undaunted and decided to pass another winter on Bowdoin Bay with a few of his stouter-hearted companions and to try again to cross the ice in the following spring. After unloading its supplies the *Falcon* left for three weeks of exploration along the coast of Ellesmere Land, before starting back south. Chamberlin thus had three weeks for a study of the glaciers of the neighborhood under a never-setting sun.

The results of this strenuous work have been presented as "Glacial Studies in Greenland," in the *Journal of Geology;* in his presidential address before the Geological Society of America in 1895, and later in more scattered form in textbooks. Glaciers in temperate latitudes, as is well known, end in slopes of moderate declivity, thinning and disappearing at their margins as a consequence chiefly of progressive surface melting. Chamberlin found that this habit prevails with the Greenland glaciers as far north as Disco Island and even somewhat beyond. But in Inglefield Gulf, between the latitudes of 77 and 78 degrees, the glaciers end in abrupt terminal walls rising to heights of 50 to 150 feet. This peculiarity he interpreted as being due to the fact that in this high latitude the sun's rays are at a low angle and are directed against the ice from all points of the compass during the continuous daylight of the summer.

Chamberlin described the pronounced stratification of the ice as the most impressive feature displayed in the vertical sides of these high-latitude glaciers. As contrasted with the glaciers of the Alps, the remarkable extent, the definiteness, and the peculiar characteristics of the stratification displayed in the vertical walls of these northern glaciers were a veritable revelation

to him. On the vertical ice faces there were characteristically two notable zones, an upper inconspicuously laminated one of nearly pure white ice, and a lower one whose laminated structure was made very conspicuous by the presence, along very numerous parting surfaces, of rocky débris ranging from fine silt to boulders. This débris was in very definite and limited horizons, leaving the ice clean and pure above and below. In general, the abundant débris was confined to the lower fifty or seventy-five feet of the glacier.

The question of foremost importance was the origin of the stratification, or foliation, and the method of introduction of the débris. "The original stratification could not have been very pronounced. Perhaps it was intensified somewhat during subsequent consolidation, but some new agency was necessary to produce the more definite partings and to introduce the layers of débris. This agency appears to have been a shearing movement between the layers."[21]

On almost every one of the vertical faces certain layers were found to jut out sharply over those beneath. The phenomenon suggested differential movement between layers, but the possibility that it had resulted instead from differences in melting raised doubts as to the truth of the explanation. More direct proof was sought in grooves or flutings which cobblestones might be expected to make if movement occurred between the layers. Such were found. But the best evidence of the verity of shearing between layers was the intrusion of the earthy material itself along planes of slippage. This process Chamberlin was fortunate enough to observe in actual operation in places where a glacier was overriding embossments of rock. That internal shearing plays an important part in the movement of glaciers seemed thus to be demonstrated.

The many other features of the well-rounded glacial studies consummated in these three weeks must be passed over, but when, on August 26, the *Falcon* turned her prow southward and Lieutenant Peary left the ship to spend another dreary winter

[21] Recent Glacial Studies in Greenland. Bull. Geol. Soc. Amer., Vol. 6 (1895), pp. 206-208.

in the Arctic, it seems to have been generally remarked that the only member of the party who had accomplished what he set out to do was Professor Chamberlin.

MULTIPLE GLACIATION IN NORTH AMERICA

The first formal attempt to define and name the multiple glaciations of North America was that of Chamberlin which appeared in 1894 in the third edition of "The Great Ice Age" by James Geikie. His classification was as follows:

Glacial Period

1. Concealed under series (theoretical)—Unknown.
2. Kansan stage of glaciation—First (represented) glacial epoch.
3. First interval of deglaciation—First interglacial epoch.
4. East Iowan stage of glaciation—Second glacial epoch.
5. Second interval of deglaciation—Second interglacial epoch.
6. East Wisconsin stage of glaciation—Third glacial epoch.
7. Later oscillations of undetermined importance—Final glacial epoch embracing possibly a fourth glacial epoch.

W J McGee had distinguished two glacial drift sheets in Iowa, an "upper till" and a "lower till," separated by a bed of interglacial accumulations not everywhere a "forest bed" but conveniently so styled. McGee had worked chiefly in northeastern Iowa, but had reconnoitered the area south and west of the Des Moines lobe of Wisconsin age and had there identified the same group of sheets which he had long studied in northeastern Iowa. Chamberlin, however, in a reconnaissance along the route of the Burlington railway in southern Iowa, had seen the extensive excavations recently made to get ballast for the road, and particularly the deep cuts made between Afton and Thayer in improving the grade across the Grand River Valley. McGee had not visited this most promising locality. Since they could not understand one another's descriptions, they held a field conference in 1893, divided between the areas of their more special studies. McGee's interpretation thus centered on north-

eastern Iowa, while Chamberlin's centered on the Grand River localities *in the south central summit region of Iowa.* These seemed to be in general harmony, though today we know that a portion of northeastern Iowa is surfaced by a younger drift not occurring in central and southern Iowa.

Shortly after this field conference, Chamberlin was asked by Geikie to prepare the chapters on the glaciation in North America for his well-known work, "The Great Ice Age." This invitation was the immediate occasion for framing and publishing the mode of interpretation, correlation, and nomenclature given above.

"At the outset this scheme recognized only three ice invasions of the major type. The bed of till lying on Paleozoic rock at the bend of the Grand River, about a mile southeast of Afton Junction, was taken as the type locality of the glacial sheet left by the first ice invasion. It was named Kansan from the belief that it belonged to the same horizon as the old patchy drift on the rise toward the Ozarks south of the Kansas River, and that it represented the farthest extension to windward of the North American ice invasions. The Kansan formation at this typical locality was also made to include the kame-like deposits shown in the four trenches nearest Afton Junction. The original description in *The Great Ice Age* (page 757) was placed under the sidehead, 'Kansan formulation.' It includes this statement, 'there occur buried lenticular masses of gravel that appear to be kames formed on the surface of this deposit (the original Kansan) and subsequently buried by till belonging to the next higher formation' (the original Iowan).

"In the original text the Aftonian (interglacial deposit between first and second drifts) was not characterized by gravels but by the direct evidence of an interglacial interval, such as soils, muck, peat, twigs, limbs, trunks, fossils, etc. (*The Great Ice Age,*" pp. 757-759). No stress has ever been laid on gravels by the proponent of the scheme of 1894. In so far as gravels were associated with these direct evidences, they were usually gravels of the horizontal stream type as already stated.

"The very exceptional exposures of the upper till in the Thayer-Afton Junction region were regarded as the product of the second ice invasion. They embrace the greatest summit formations in Iowa and constitute the most representative glacial formation of that state. Even in 1894, it was practically known

to occupy a wide belt at the surface embracing and running under the Des Moines lobe of the third (Wisconsin) ice invasion."[22]

"In the two years following the publication of the correlation of 1894, it was shown by reconnaissances that the upper till sheet, the original Iowan, extended farther south than it was represented on the map in *The Great Ice Age*. Bain held, on the basis of a reconnaissance, that it extended to the state of Kansas. This indicated that the second ice invasion was more nearly co-extensive with the first than had been assumed with unnecessary conservatism. This increased the importance of the Iowan and added weight to the priority that had been given it. It was pre-eminently worthy to bear the state name as it was the most outstanding glacial formation in Iowa.

"Up to this time, it had been natural, as well as conservative, to assume, in the absence of evidence to the contrary, that the old glacial sheets in Illinois were contemporaneous with the old sheets in the trans-Mississippian region. But almost simultaneously with the issue of a three-fold division of the system of 1894, Leverett found that one of the older ice sheets of Illinois had crossed the Mississippi and overlapped the upper sheet of the west side. He also found evidence of a notable interval between the overlapping sheet and the formation of the preceding surface. This was fully confirmed later, and in 1896 the Illinoian invasion was added to those previously named, making four in all.

"As these four glacial invasions are all that now (1928) seem worthy of recognition, it will help to give definiteness to our sketch of the history of the shifting of names that followed, to give the four here in tabular form.

4. The fourth glacial invasion, Wisconsin.
3. The third glacial invasion, Illinoian.
2. The second glacial invasion, Iowan.
1. The first glacial invasion, Kansan."[23]

Most unfortunately the two years following the publication of the system of 1894 in "The Great Ice Age" were marked by so many shifts of terms and correlations that it is difficult for the ordinary reader or the busy geologist to follow the history

[22] T. C. Chamberlin: The Development of the Theory of Multiple Glaciation in North America. (Unpublished manuscript, March, 1928, pp. 16-18.)

[23] Ibid., pp. 19-20.

with accuracy. The shifts followed one another in almost kaleidoscopic frequency and complication.

The attempted emendations began in 1895 with the recognition by Calvin that the surface drift in the northern portion of Johnson County, in east central Iowa, was much younger in age than the drift surfacing the southern portion of the county, and that there were thus three instead of two pre-Wisconsin tills in McGee's area. In the northern part was a thin, patchy drift of young aspect, covering the much thicker drift (the middle sheet of the three-fold division) which was the surface formation to the south.

McGee had used the terms "lower till," "forest bed," "upper till." Chamberlin's scheme used Kansan, Aftonian, Iowan, terms not used previously. Chamberlin always believed firmly that McGee's "upper till" was, in the main, the great surface sheet of southern Iowa (the second drift or upper till in Chamberlin's type section near Afton), but included also in northeastern Iowa the thin local cover sheet which Calvin, in 1895, differentiated as the third drift. His own system of 1894 took no cognizance of this cover sheet, which came to be appreciated later and which is confined to northeastern Iowa, but only recognized the main mass of McGee's upper till, keeping to the fore the splendid exposures in the Afton region, far beyond the southern limits of the cover sheet. He had temporarily attached the prefix *East* to the state names, Wisconsin and Iowan, as a prudential measure to guard against a possible misuse of the unqualified state names, for he felt that only the best exhibit of a given state should, as a rule, bear the unqualified state name, and that too little was then known of the glacial formations of northwest Wisconsin and western Iowa to warrant the unqualified use of the names Wisconsin and Iowan for formations which had only been intensively studied in the eastern halves of these states. Later he regretted this excessive scrupulousness, as the prefixes were only so much lumber and even led frequently to misinterpretation. They were soon discarded when no longer needed.

The Iowa Geological Survey began in 1895 to give particular attention to the local cover sheet and adopted the view that McGee's upper till was merely this cover sheet of northeastern Iowa rather than the great sheet on which it rests and which is the surface drift over a large part of the state beyond the confines of the cover. The Survey then went further and proceeded to use Chamberlin's term Iowan for the cover sheet and his term Kansan for the greater sheet below and beyond, which Chamberlin had called Iowan. This use of Chamberlin's names was then given much currency in the Iowa county reports which were coming out in rapid succession. Bain later went to the Afton region and found that these names, as the Iowa geologists were then using them in northeastern Iowa, did not fit Chamberlin's type sections. He might now have recognized the Afton sections as the basis of Chamberlin's published nomenclature, and in accordance with the claims of priority have brought the newer developments in northeastern Iowa into harmony with it. Instead he shifted Chamberlin's names to fit his own ideas of northeastern Iowa, transferring the name Kansan, which Chamberlin had used for the first drift, to the second drift which Chamberlin had named Iowan, thus leaving the first drift without a name and divorcing the term Iowan from its type sections near Afton and Thayer. The Iowa county reports then came to use the succession, sub-Aftonian or pre-Kansan, Kansan, Illinoian, Iowan, and Wisconsin.

Perhaps nothing showed more plainly the restraint, patience, and generously friendly character of Chamberlin than his reception of these developments. He revisited Thayer and Afton with Bain, who later was to take graduate work and the doctor's degree under him at Chicago, and also went over other parts of Iowa as guest of the State Survey. The problems were complicated and confusing, and he was able to appreciate alternative points of view. In a very generous spirit he acquiesced, though inwardly with considerable misgiving. In later years, when he saw the fuller relationships of the glacial succession and was convinced that a very unfortunate mistake had been made, he

deeply regretted having allowed his kindly impulses to overcome his judgment.

The difficulty had arisen from a difference of opinion as to the proper interpretation of McGee's lower and upper till with separating forest bed, which even today is a debatable question. More particularly it arose from making an interpretation so open to doubt the basis of a correlation and nomenclature to which the whole American glacial succession was to be adjusted. The uncertainty of the premises may be illustrated by quoting from a paper by Calvin in 1898:

"McGee looked upon the forest bed as the plane of division between his lower and upper till, but later investigators following the lines which he pointed out have reached the conclusion that his lower till embraces two distinct drift sheets, and that it is between these two that the forest bed invariably lies. Thus there are three drift sheets in northeastern Iowa, and in the recent literature referring to Pleistocene geology they are known respectively as sub-Aftonian or Albertan, Kansan and Iowan. No forest material has been observed between the Kansan and the Iowan, but in this situation there occur extensive beds of stratified sands and gravels."[24]

"Calvin believed that in northeastern Iowa the forest bed was 'invariably' between the sub-Aftonian and Kansan tills."[25]

Though the names have been shifted, this recognition that the forest bed, as then known, was between the first and second drifts and not between the second drift and the cover sheet is in perfect accord with Chamberlin's classification. The cover sheet, not recognized by Chamberlin but subsequently differentiated, accordingly becomes a separate unit to be named and fitted into its proper place in the sequence. More recently patches of forest bed have also been found between the second drift and the cover sheet.

In the cut of the Chicago Great Western Railroad near Oelwein, in northeastern Iowa, were found three drifts with a dis-

[24] Samuel Calvin: The Interglacial Deposits of Northeastern Iowa. Iowa Acad. Sci., Vol. V (1898) pp. 64-70.

[25] George F. Kay: History of the Investigation and Classification of the Pleistocene Deposits of Iowa. Iowa Geol. Surv., 1928, p. 20.

tinct forest bed between the lowest drift and the intermediate drift. Bain wrote: "A review of sections published by McGee makes it more than doubtful whether the forest bed which he has so clearly shown to be present does not mark the Aftonian rather than the Buchanan horizon between the two upper tills."[26]

Kay, however, thinks that there is good argument in defense of the policy followed by the Iowa Geological Survey, if proper emphasis is given to McGee's description of his two tills, their mapping, and his forest bed or gumbo, rather than too great stress given to his forest bed alone. He continues: "At the time the Oelwein cut was described gravels and sand only had been found by Calvin and his assistants in sections separating the Iowan from the Kansan, whereas McGee emphasized the number of well records in which a forest bed separated his Lower Till from his Upper Till. Since, as stated above, it is now known that in McGee's area there is in places a forest bed at the same stratigraphic horizon as the Buchanan sands and gravels and a forest bed at the stratigraphic horizon of the Aftonian, quite naturally McGee's forest bed in some of his sections is the forest bed between the Kansan and the Iowan, as these names are now applied, and in others the forest bed lies between the Kansan and the pre-Kansan."[27]

Here would seem to be the probable truth of the matter. In some places McGee's upper till was the second drift; in other places it was the second drift plus the cover sheet where that was also present; in still other places it was probably merely the cover sheet. Stratigraphically, McGee was lumping together in many places two distinct drifts, a thick and a thin one; topographically, he noted more particularly the characteristics of the thin local cover in northeastern Iowa. In this pioneer grouping together of different glacial units lay the germ of the unfortunate proceedings which followed. Nothing composite, however, characterized Chamberlin's Grand River section near

[26] H. F. Bain: Relations of the Wisconsin and Kansan Drift Sheets in Central Iowa, and Related Phenomena. Iowa Geol. Surv., Vol. VI, 1896, pp. 466-467.

[27] George F. Kay: Op. cit. p. 21.

Afton Junction in south central Iowa, which showed the first drift, which he called Kansan, and the second drift, which he called Iowan and without a later drift to cause confusion. But still the Iowa Survey shifted the very appropriate nomenclature of the splendid Grand River type section to fit the debatable interpretations farther north.

In the years which have followed, the comparatively insignificant cover sheet, carrying the name Iowan, has been an embarrassing member of the North American glacial succession and has occupied an uncertain position. The U. S. Geological Survey party engaged in correlating the older drifts around the driftless area in 1906-07 even tested out carefully the hypothesis that there was *no such distinct drift sheet,* but in the end became convinced of its existence. Later it was regarded by some as possibly the Keewatin equivalent of the Illinoian. But the very recent studies of Kay and Leighton seem now to demonstrate that it represents an early advance of the last great ice invasion, which we call the Wisconsin.

Thus, by a poetic irony of fate, the second drift which is *the* great drift sheet of the state of Iowa is now known as the Kansan, and the so-called Iowan glaciation of the Iowa geologists is apparently now to be taken out of the category of major ice invasions and reduced to a sub-stage of the Wisconsin. With four instead of the anomalous five major ice invasions, the American glacial succession comes into harmony with that of Europe.

Chamberlin did not live to see this last development, but in his later years he cherished the hope that respect for the claims of priority, coupled with the greater appropriateness of his terms, would lead to an ultimate return to the sequence, 1. Kansan, 2. Iowan, 3. Illinoian, and 4. Wisconsin, leaving the status of the cover sheet to be adjusted later. Iowa would then be restored to the prominence and prestige in Pleistocene nomenclature to which the state is so justly entitled. In his last year he started steps to bring this about, and had he lived longer his leadership might, perhaps, have accomplished it.

GEOLOGIC CLIMATES

Glacial geology had been Chamberlin's chosen field up to the middle nineties of the last century. By this time field studies had given a fair understanding of the main features of the Pleistocene glaciation of the country, and competent observers were actively engaged in advancing this knowledge through systematic areal investigations. Chamberlin's interest now spread to the larger scientific implications of glaciation. He turned in particular toward the causes of glacial climates which naturally led to a broader consideration of the changing climates of the geologic past. These, in their turn, were to prove but the first steps of that long flight of stairs which he was to mount steadily for thirty years till he reached the heights in cosmogony and fundamental geologic philosophy. The ascent throughout was laborious and difficult, but each step led directly to the next. The whole was a sustained effort remarkable alike for steady systematic progress into the unknown and for the far-reaching results finally achieved.

He began by considering the earth's atmosphere. "All our attempts at the solution of climatic problems proceed on some conscious or *unconscious* assumption concerning the extent and nature of the atmosphere at the stage involved."[28] Accumulating geologic evidence seemed to him to indicate no radical differences between the atmosphere in Paleozoic time and that of the more recent ages. Very opportunely signs of glaciation far back in the Paleozoic were then beginning to be found, and Chamberlin was quick to grasp their great significance. Belief was then nearly universal in an earth gradually cooling off from an original fiery state. The Laplacian nebular hypothesis had led directly to the view that the earth's early atmosphere was an enormous mass of hot vapors and gases surrounding a molten globe. Fifty times our total present atmosphere was the estimated amount of atmospheric carbon dioxide later to become locked up in our extensive carbonate rocks and coal deposits. The great hot primordial atmosphere was thought to have di-

[28] "A Group of Hypotheses Bearing on Climatic Changes." Jour. of Geol., Vol. V (1897), p. 653.

minished to its present volume with the passage of time and earth climates to have become progressively cooler. But glaciation early in the earth's history and the vast original atmosphere, rich in carbon dioxide, postulated in accordance with the Laplacian hypothesis, were apparently incompatible. Chamberlin then examined the postulate of a once-molten globe with heavy uncondensed atmosphere and then, in turn, the basis for it in the nebular hypothesis at that time practically universally accepted. But the careful scrutiny which he gave the nebular hypothesis developed unsuspected weaknesses of a serious nature. As the foundations of the hypothetical structure began to crumble before his analysis, Chamberlin boldly ventured to question not only the verity of the supposed original molten state of the earth but the correctness of the time-honored Laplacian hypothesis itself.

But he was not one to tear down without trying to build something better in its place. Even in his 1897 contribution on climatic changes there was already entertained the alternative hypothesis that the earth may have grown as a solid body by slow accretion of meteoroidal material. A source of the earth's heat was found in self-compression through rearrangement of this heterogeneous material under the force of gravity. From within such an earth the atmosphere could be resupplied by volcanic and other exhalations in proportion as its gases were lost. Through losses and resupply, an atmosphere fluctuating only moderately about an equilibrium value seemed to be possible, and a vast, hot atmospheric envelope, with all its attendant geologic consequences, was therefore no longer a necessary deduction.

Current conceptions, both of the early atmosphere of the globe and of the origin of the earth itself, were plainly subject to the gravest doubt; alternative hypotheses to replace them were already in view. The way was thus opened for a study of geologic climates on a new basis, and a better theory for the origin of the solar system became one of the greatest needs of geology. To both of these great problems Chamberlin now directed his attention. The climatic studies matured first.

Some fifty years earlier Tyndall had suggested that glaciation might be dependent upon depletion of the atmospheric carbon dioxide whose peculiar competence to retain solar heat he had demonstrated. This suggestion, however, had failed to find much acceptance, partly from doubt as to its adequacy and partly from lack of any definitely assignable cause for the requisite intermittent depletion. Arrhenius, in 1896, endeavored to ascertain what degree of depletion of the carbon dioxide of the present atmosphere would bring on the conditions of Pleistocene glaciation and what degree of enrichment would produce the warm climate of the Tertiary. He reached the conclusion that the removal of 38 to 45 per cent of the present carbon dioxide would bring on glaciation, whereas an increase of 2.5 or 3 times its value would produce the mild temperatures of Tertiary times.

Chamberlin investigated the geologic setting and supplied the possible mechanism. Following the greater diastrophic revolutions the lands are relatively high and the land surfaces of the globe reach their greatest areal extent. At such times of unusually extensive exposure of the rocks, carbon dioxide from the atmosphere enters into chemical combination with the rocks, in the process of carbonation of the silicates, to an exceptional degree. This reduction in the amount of carbon dioxide in the atmosphere lowers its temperature by permitting more rapid radiation of the sun's heat from the earth. The oceans in turn become cooler and consequently absorb more carbon dioxide from the atmosphere. This further removal of carbon dioxide from the atmosphere results in a still further lowering of its temperature. Water vapor in the atmosphere also plays its appropriate part, and other processes are involved. Cold climates result.

On the other hand, during times of baseleveling, when shallow seas have spread widely upon the continents protecting great areas of rock from carbonation, carbon dioxide is freed in large quantities as the calcium bicarbonate in the sea water becomes changed to the monocarbonate in the development of the exten-

sive limestone deposits so characteristic of such times.[29] With enrichment of the atmospheric carbon dioxide the earth is favored with warm, equable climates, such as have prevailed during much of geologic time. During such epochs the geologic record shows that temperate and tropical faunas and floras have spread strongly poleward.

The fluctuating climates of the earth manifestly have resulted from the interplay of a large number of variable factors. The extreme complexity of the processes in operation and the delicate adjustments that maintain a general equilibrium and have permitted fluctuation only between comparatively narrow limits of temperature made this a problem requiring for its solution the fullest understanding of what was involved and the keenest analysis. In 1900, Chamberlin presented his study of the intricacies of this problem and his working hypothesis of the important part played by fluctuating atmospheric carbon dioxide.[30]

An auxiliary agency in the control of secular climates appeared in possible reversals of the direction of water movement in the deeper portion of the oceans.

"In an endeavor to find some measure of the rate of the abysmal circulation, it became clear that the agencies which influenced the deep-sea movements in opposite phases [poleward or equatorward] were very nearly balanced. From this sprang the suggestion that if the relative values were changed to the extent implied by geological evidence there might be a reversal in the direction of the deep-sea circulation and that this might throw light on some of the strange climatic phenomena of the past and give us a new means of forecast of climatic states in the future."[31]

The most important opposing agencies are differences in temperature, including freezing and thawing, and differences in

[29] "The Influence of Great Epochs of Limestone Formation upon the Constitution of the Atmosphere." Jour. of Geol., Vol. VI (1898), pp. 609-21.

[30] "An Attempt to Frame a Working Hypothesis of the Cause of Glacial Periods on an Atmospheric Basis." Jour. of Geol., Vol. VII (1899), pp. 545-85.

[31] "On a Possible Reversal of Deep-Sea Circulation and Its Influence on Geologic Climates." Proc. Amer. Phil. Soc., Vol. XLV (1906), pp. 33-43.

salinity of the ocean waters. In so far as evaporation exceeds precipitation in the low latitudes, it results in an increased salinity of the superficial waters and a tendency of these to sink and flow poleward. On the other hand, the lower temperatures of the high latitudes, and the exclusion of salts from freezing seawater, increase the density of the water and tend to cause it to sink and flow equatorward. At the present time the polar agencies control the deep-sea circulation; the deep waters, even beneath the tropics, are cold, and the climate is rigorous and diversified. Although today we enjoy considerable amelioration of the more severe glacial climates of not long ago, the deep-sea circulation still remains equatorward as presumably it was also throughout the Pleistocene glacial epoch.

Chamberlin's survey of the existing temperatures and salinities of the ocean made it clear that the battle between temperature and salinity is a close one and that no profound change is necessary to turn the balance. To him it seemed that the lowering of lands by the baseleveling processes with resulting spread of the seas on the continents and accompanying warming of the atmosphere, partly through increased carbon dioxide and partly due to other causes, would be likely to render evaporation and salinity dominant and reverse the deep-sea circulation. Warm waters descending in the equatorial regions, moving poleward and rising to the surface in the high latitudes would constitute a veritable hot-water heating system delivering heat in the polar regions and giving the whole globe the mild, equable climates which the geologic evidence indicates have prevailed, interspersed with occasional epochs of glaciation, through most of the geologic past.

The correlation of climates with the pulsations of geologic history, specifically the relatively short diastrophic upheavals and the longer intervals of baseleveling and sea-transgression is beautifully shown by the geologic record. The transcendent importance of these relations in any adequate concept of the typical geologic period, either from the point of view of the physical history or that of the biologic response to the physical conditions, should have been apparent long ago. Yet very few

geologists seem to have realized this. Few, indeed, have any convictions, pro or con, on the solidarity of the hypothesis of the reversal of the deep-sea circulation, presented a quarter of a century ago. If true, it was a contribution of far-reaching significance and should play a vital part in fundamental geologic philosophy; if faulty, there is still a great gap entirely unfilled.

THE REPLY TO LORD KELVIN

In 1897 Lord Kelvin delivered his famous address before the Victoria Institute on "The Age of the Earth as an Abode Fitted for Life." In that address he took up the question of loss of heat from a cooling molten earth and the longevity of the sun as a highly heated body, and from his analytical studies asserted with a finality unusual in scientific discussions that the age of the earth was more than twenty and less than forty million years, and probably much nearer twenty than forty million years. Geologists and biologists had been requiring much greater lengths of time for the recorded history of the earth and the evolution of its inhabitants. To be denied this seemingly necessary span of time was disconcerting, and yet so great was the prestige of Lord Kelvin in the field of mathematical physics that the age limits which he placed on the earth and its evolution had scarcely less force than a supreme court decision.

Geologists, according to Kelvin's conclusions, had been very extravagant in their time postulates. Possibly the scientific public carried the implications even further, for the address had been "permeated with an air of retrospective triumph and a tone of prophetic assurance." At best the geologists were in a position of uncertainty between the dictum of exact science on one hand and faith in their own methods and criteria on the other.

For some reason Kelvin's address was reprinted in *Science* in May of 1899. Chamberlin now promptly and boldly challenged Lord Kelvin along the whole front of his argumentative position.[32] Not only did he show the weaknesses of his geologic

[32] Science. Vol. IX (1899), pp. 889-901 and Vol. X (1899), pp. 11-18.

assumptions, but he took him to task more particularly on the basis of physical laws and physical antecedents. The stroke was as unexpected at that time as today it seems remarkable in the light of subsequent advances in physics. Not a few British scientists expressed surprise that anybody should have the temerity even to reply to Lord Kelvin, let alone to attempt to unhorse him in his own field. Yet the latter is precisely what was done, in ways that can be appreciated much better now than at that time.

Let us follow merely Kelvin's third line of argument which was drawn from the theoretical age of the sun. Accepting the Helmholtzian theory that the sun's heat is solely the result of its contraction, Kelvin calculated that the sun could not have supplied sufficient heat for life on the earth for more than twenty to thirty million years. Let us now read Chamberlin's reply, keeping in mind as we do so that it was given before any of the remarkable developments in physics during the last few years, particularly the discovery of electrons, the complex structure of atoms, and the prodigious amounts of energy now known to be locked up in atomic structure. With our present concepts of atomic physics in mind, we may appreciate the extraordinarily prophetic vision developed in the following paragraph which Chamberlin wrote in 1899.[33]

"Here is an unqualified assumption of the completeness of the Helmholtzian theory of the sun's heat and of the correctness of deductions drawn from it in relation to the past life of the sun. There is the further assumption, by implication, that no other essential factors entered into the problem. Are these assumptions beyond legitimate question? In the first place, without questioning its correctness, is it safe to assume that the Helmholtzian hypothesis of the heat of the sun is a complete theory? Is present knowledge relative to the behavior of matter under such extraordinary conditions as obtain in the interior of the sun sufficiently exhaustive to warrant the assertion that no unrecognized sources of heat reside there? What the internal constitution of the atoms may be is yet an open question. It is not improbable that they are complex organizations and the seats of enormous energies. Certainly, no careful chemist would

[33] Science. Vol. X (1899), p. 12.

affirm either that the atoms are really elementary or that there may not be locked up in them energies of the first order of magnitude. No cautious chemist would probably venture to assert that the component atomecules, to use a convenient phrase, may not have energies of rotation, revolution, position and be otherwise comparable in kind and proportion to those of a planetary system. Nor would he probably feel prepared to affirm or deny that the extraordinary conditions which reside in the center of the sun may not set free a portion of this energy. The Helmholtzian theory takes no cognizance of latent and occluded energies of an atomic or ultra-atomic nature. A ton of ice and a ton of water at a like distance from the center of the system are accounted equivalents, though they differ notably in the total sum of their energies. The familiar latent and chemical energies are, to be sure, negligible quantities compared with the enormous resources that reside in gravitation. But is it quite safe to assume that this is true of the unknown energies wrapped up in the internal constitution of the atoms? Are we quite sure we have yet probed the bottom of the sources of energy and are able to measure even roughly its sum-total?"

This sounds like inspired prophecy! Substitute our present word "electrons" for Chamberlin's "atomecules" and the vision of 1899 is almost uncanny in its prophetic correctness. That Chamberlin should thus have anticipated, by almost a quarter of a century, in form as well as in substance, some of the most basal concepts of modern physics was, as Professor Moulton has recently said,[34] in some sense accidental, but that he saw that there were these energies, that there *must be* some such energies, instead of being accidental, was the result of mature reflection upon a whole range of phenomena whose import he saw with remarkable clearness.

Even in 1899, when the full significance of this reply could not be appreciated, its effect on geologic thought was prompt and decisive; Lord Kelvin's time restrictions never bothered geologists thereafter. Significant also was the reply in showing how far its author had already advanced in those fundamental researches which later grew into an almost complete geologic philosophy.

[34] F. R. Moulton: The Planetesimal Hypothesis. Science, Vol. LXVIII (1928), pp. 10-12.

THE PLANETESIMAL HYPOTHESIS

The study of ancient geologic climates disclosed facts which seemed to Chamberlin inconsistent with the common belief growing out of the Laplacian hypothesis that a hot, vaporous atmosphere of enormous extent surrounded the earth during its early stages. Tested by means of the molecular velocities, after the method of Johnstone Stoney, the supposed vast atmosphere was found to be an impossibility. This conclusion naturally led back one step further to the question of whether there was a basis for the view in the Laplacian nebular hypothesis, and eventually raised question as to the correctness of that time-honored theory of the beginning of the earth.

The earth-moon ring postulated by Laplace was put to the test and it was found that this supposed ring could not remain in a true gaseous state under the conditions required by the hypothesis. Chamberlin, therefore, began to consider the alternative possibility of molecules or particles revolving in independent orbits in planetoidal fashion. Condensation from such a state had been generally held, though without sufficient reason, to give rise to retrograde rotations, whereas most of the rotations of the solar system are forward. But Chamberlin, by a further analysis of the problem found grounds for concluding that, on account of the ellipticity of the orbits, forward rotations will in general result from the gathering together of individual particles. The importance of this conclusion was very great for it indicated the tenability of an alternative method of earth genesis. Instead of a single mode of genesis, it now became a question of which of two alternatives was better in accord with molecular behavior and recognized mechanical principles.

The next step was to apply critical tests to the Laplacian hypothesis along as many different lines as possible. Professor F. R. Moulton, who had been collaborating with Chamberlin from an early stage in these cosmogonic studies, brought to bear his mastery of celestial mechanics and his mathematical insight and contributed brilliantly to the success of their joint investigations. The two men were very different in their train-

ing, points of view, and methods of analysis. One was naturalistic, the other mathematical; together they made an ideal team, each supplementing the other. They worked so closely and informally together that the precise parts which each played in disproving the Laplacian hypothesis and in developing the planetesimal hypothesis to replace it are not readily separated and will not be differentiated here. They have been unscrambled to some extent by the authors themselves in a joint statement called forth by certain not over-careful criticisms and captures by other workers in related fields.[35] The credit belongs to both.

The searching tests by Chamberlin and Moulton proved conclusively the untenability of the Laplacian hypothesis. This famous theory failed to meet the requirements along a dozen different lines, and they formally and finally abandoned it in 1900. But so firmly entrenched in the scientific thought of the time was this beautifully harmonious hypothesis, and so great is the inertia of inherited ideas, that Chamberlin frequently remarked that he expected a quarter of a century would elapse before the geological and astronomical professions as a whole would finally give it up for something better. It is interesting to note that the date given in Moulton's new Astronomy (1931) for the general abandonment of the Laplacian hypothesis is 1925.

On the constructive side considerable futile work was done, largely by Chamberlin, in trying out the possibilities of collision between nebulous bodies, "but no escape was found from the high probability, amounting almost to certainty, that the resulting orbits would be too eccentric to fit the case of the solar system in any instance that was likely to occur."[36] He proceeded to a study of the effects of the differential attractions exerted by bodies on one another when they make close approaches, as developed by the researches of Roche, Maxwell, and others, and

[35] "The Development of the Planetesimal Hypothesis." Science, Vol. XXX (1909), pp. 642-45. This statement has been reprinted together with the seven attacks and counter attacks which preceded it (Science, April 23, 1909, to November 5, 1909) under the title: "Our Friends, the Enemy, a Discussion bearing on Scientific Ethics, with Concrete Illustrations." Idem. (1909), pp. 21.

[36] Science, Vol. XXX (1909), p. 644.

found this a promising field. Not only was the chance of close approach vastly greater than that of actual collision, but in addition to the direct tidal effect he recognized also "the projective effect developed in a body of enormous elasticity already under high pressure and affected by violent local explosions which were subject to intensification by the changes of gravity brought to bear on them by a passing body.[37] The disruption of solid bodies was seen to afford a basis for explaining the clustered fragments of comets and the angularity of the meteorites into which they are supposed to be finally dispersed. The explosive projections from suns under the influence of a passing body were thought to explain the two-armed spiral nebulæ which were then coming into prominence as the dominant type of nebula in the heavens.[38]

Following these preliminary steps, which cleared the way by narrowing the limitations of tenable hypotheses and which defined and developed the governing principles, the planetesimal hypothesis was gradually molded into the form in which it appeared in 1904 in Year Book No. 3 of the Carnegie Institution. When it appeared somewhat further elaborated for textbook use in Chamberlin and Salisbury's "Geology," Volume II, at the beginning of 1906 and in Moulton's "Introduction to Astronomy" in the same year, the planetesimal hypothesis for the origin of the solar system was fully launched on its career.

At that time spiral nebulæ played an important part in the planetesimal hypothesis. Known then to be the dominant nebulæ of the heavens, it was at first thought that they might represent huge solar systems in the making. Their characteristic large central mass, from which two arms of scattered haze, studded with large and small knots of matter, curved spirally outward on opposite sides, seemed just what an embryo solar system ought to be according to the planetesimal hypothesis. The central mass seemingly would become the

[37] Idem.

[38] "On a Possible Function of Disruptive Approach in the Formation of Meteorites, Comets and Nebulæ." Astrophys. Jour., Vol. XIV (1901), pp. 17-40 and Jour. of Geol., Vol. IX (1901), 369-92.

sun; the larger knots of matter in the arms would become planets by gathering in the innumerable planetesimals constituting the general haze, while the smaller knots would give rise to satellites and planetoids. Diagrams showed how a star passing close to our sun would cause the development of a nebula of this spiral form, but it was explicitly stated that the planetesimal hypothesis was in no way dependent on the spiral form or upon the nature of the great spiral nebulæ revealed by the telescopes. The only essential feature was that the passing star pulls less than one per cent of the matter of the sun into elliptical orbits around it.

This was the stem hypothesis; from it the peculiar conditions of any individual case might cause a solar system to evolve in accordance with any one of a considerable number of sub-hypotheses, all of them species of the same genus. Much time, in fact, was given to testing out various alternative sub-hypotheses. When, some years later, further researches with larger telescopes showed the spiral nebulæ of the heavens to be vast independent galaxies of stars beyond the limits of our own galaxy, the planetesimal hypothesis was in no way adversely affected by this advance of knowledge. In fact, the supreme advantage of the planetesimal hypothesis lies in having opened an entirely new field, wide and fertile in possibilities, and from which in all probabilty the true explanation of the solar system either has sprung or will spring. It may be that some other of the sub-hypotheses will be found to work out better, as knowledge develops, than the particular one to which the authors have given preference. But as yet their selection seems best.

CHAMBERLIN AND SALISBURY'S "GEOLOGY"

Chamberlin's chosen lines of research had led him into steadily widening fields and these he was now plowing deeply. The planetesimal hypothesis provided for an earth with inherited characters quite unlike those previously postulated. Not only did the new view assign to it double parentage, instead of only one parent, but gave it also very different environ-

mental conditions during the formative stages of its youth when its characteristics were being established. Naturally the better-known eras of its later life were but the outgrowth of the juvenile earth and were to be interpreted in accordance with its antecedents. A new geologic philosophy therefore sprang from the planetesimal hypothesis, and its far-reaching ramifications were to occupy Chamberlin throughout the rest of his life.

The development of these new lines was an important feature of the Chamberlin and Salisbury "Geology" whose three volumes came out between 1904 and 1906. The publishing house of Henry Holt and Company had sought this textbook for their American Science Series some time before the planetesimal hypothesis appeared, and the text was already under way in 1900. But as the new cosmogony unfolded and many geologic innovations were found to spring from it, the authors set themselves the task of bringing out a comprehensive textbook of geology developed along the new lines.

Volume I treated geologic processes to which Salisbury contributed most largely on erosion and deposition, and Chamberlin most largely on diastrophism, vulcanism, and the origin and descent of rocks.

Volumes II and III gave the history of the earth. Chamberlin portrayed in the first two chapters the origin of the solar system and the early stages of the earth, sketching in parallel fashion the development of the earth to maturity, first as had been taught in accordance with the Laplacian hypothesis and then as logically developed from the planetesimal hypothesis. It was characteristic of the spirit and method of the two authors that they made every effort to amend the molten-globe story so as to accord better with recognized geologic facts, but seemingly with little success. The planetesimal evolution was then traced through the initial volcanic, atmospheric, hydrospheric, and life stages to the recognized Archeozoic era. These stages involved new conceptions of the cause of vulcanism, of the origin of the atmosphere, of the origin of continents and ocean basins, of megadiastrophism, and of the inception of life on

the globe. The departure from earlier views was revolutionary in the highest degree, but so defective did the time-honored interpretations seem to the author that the new basis was formally adopted and the Laplacian hypothesis and its consequences were given no further consideration in the unfolding of the earth's later history.

From the Cambrian period onward Salisbury was primarily responsible for the physical evolution and Chamberlin for the biological evolution. Paleontology is remote from cosmogony, but for the treatment of the faunas and floras recorded in the geologic strata Chamberlin was quite well prepared. The Wisconsin Survey while he was state geologist had done much paleontological work, and his early diagnosis of the Niagaran coral reefs even to this day excites the admiration of those who give attention to these peculiar structures. He had also published, in 1898, "A Systematic Source of Evolution of Provincial Faunas," and in 1900, "On the Habitat of the Early Vertebrates."

In the former publication he pointed out the fact that earth readjustments which cause the emergence of continents and the withdrawal of marine waters into the ocean basins are periodic (or episodic) in their occurrence, and that these periods of activity are separated by long intervening stages of relative quiescence during which lands are lowered by erosion and shallow seas are gradually spread over the continental platforms. During such a time of extended "epicontinental seas," as he termed them, the conditions for what he designated the "expansional evolution" of shallow-water marine life are signally favorable. Later, after long-continued baseleveling and expansional evolution of marine life, diastrophism causes the withdrawal of the marine waters from the continental platforms, leaving around each only a narrow, rapidly shelving shore-tract between the land and the abysmal slopes. The shallow-water fauna is compelled to follow the retiring sea and to crowd itself into this restricted zone.

"The new conditions will be in many respects uncongenial, for the streams will be rejuvenated and the amount of land

wash will be greatly increased. Those species whose existence is dependent upon clear seas will be in imminent danger of extinction. Certain species to which these conditions are congenial may on the other hand be favored, but the grand result must necessarily be the destruction of the larger part of the previous expansional fauna and the forced adaptation of the remainder to new, and on the whole sterile and hostile conditions. A stage of general repressional evolution is thereby inaugurated and, in a comparatively short period, it is safe to assume, all or nearly all preceding species will have passed out of existence and new species, in a much more limited number but better adapted to the new conditions, will have been introduced." [39]

As a result of warping, the continental platform is likely to be bordered by various embayments favorable to life, separated from one another by narrow shore tracts which largely prevent free migration of shallow-water life. Each embayment will therefore develop its fauna in measurable independence and each will become the generating area of a provincial fauna. If a time of quiescence and baseleveling now ensues, these embayments will grow into extensive gulfs and, as they join in a general epicontinental sea, the commingling of their expanding faunas may result in a new cosmopolitan assemblage. These principles entered notably into Chamberlin's discussion of the Silurian and Devonian faunas in Volume II of the textbook.

The textbook also echoed and built upon Chamberlin's classic "Habitat of the Early Vertebrates." In a few pages of effective flowing language he had portrayed the appearance of the fishes, the first known vertebrates, as one of the most abrupt and dramatic in the life history of the earth, and had proceeded to show in simple fashion, as if the whole story had been revealed to him, how perfectly the known facts fitted together to indicate that the earth's vertebrate animals had their beginning not in the waters of the ocean, as generally had been supposed, but in running waters on land.[40] The geological facts bearing

[39] Journal of Geology. Vol. VI (1898), pp. 604-05.

[40] "On the Habitat of the Early Vertebrates." Jour. of Geol., Vol. VIII (1900), pp. 400-412.

on the problem were not so much marshalled in argumentative support of a thesis to be established as they were unfolded in proper succession, along with zoological observations, to give a clear picture which invited acceptance through its own inherent probability and remarkable harmony throughout.

In flowing water, an animal must maintain its position against the current either by a contract of some resisting kind with the bottom of the stream, or by an effective mode of propulsion competent to meet the constant force of the current without undue draft on its vital resources. Otherwise it would be swept out to sea and its race would be ended as a stream-dweller. It is different with ocean currents, for they return upon themselves and an animal may yield to them without losing its marine habitat; moreover, they are usually much feebler than river currents.

"A compact form of body presents obvious advantages, except as environment or food or locomotion requires some departure from it, and the vast majority of animals are more or less rotund, and their locomotive devices are adjusted to this form. But the rotund form offers much resistance to rapid currents and unfits the animal for effective stream life unless it persistently hugs the bottom. Neither the rotund floaters and swimmers like the ancient cephalopods, nor the cilliated sprawn of the sessile forms are well adapted to resist the unceasing pressure of a rapid stream, and these are practically absent from river faunas.

"There is only one conspicuous type that is facilely suited to free life, independent of the bottom, in swift streams, and that is the fish-form. The form and the motion of the typical fish are a close imitation of the form and motion of wisps of water-grass passively shaped and gracefully waved by the pulsations of the current. The rhythmical undulations of the lamprey which perhaps best illustrates the primitive vertebrate form, and is itself archaic in structure, are an almost perfect embodiment in the active voice of the passive undulations of ropes of river confervæ. The movement of the fish is produced by alternate rhythmical contractions of the side muscles, by which the pressure of the fish's body is brought to bear in successive waves against the water of the incurved sections. In the movement of a rope of vegetation in a pulsating current, it is the

pressure of the pulses of water against the sides of the rope that give the incurvations. The two phenomena are natural reciprocals in the active and passive voices.

"The development in the fish of a rhythmical system of motion responsive to the rhythm impressed upon it by its persistent environment and duly adjusted to it in pulse and force, is a natural mode of neutralizing the current force and securing stability of position or motion against the current, as desired. Beyond question the form and the movement of the typical fish are admirably adapted to motion in static water and that has been thought a sufficient reason for the evolution of the form, and so possibly it may be, but fishes in static water have not as uniformly retained the attenuated spindle-like form and the extreme lateral flexibility as have those of running water. Among these latter it is rare that any great departure from the typical 'lines' and from ample flexibility has taken place, while it is not uncommon in sea fishes. Among the latter not a few have lost both the typical form and the flexibility. The porcupine-fish, the sea-horse, the flounders, and many others are examples of such retrogressive evolution, which is doubtless advantageous to them within their special spheres in quiet waters, but would quite unfit them for life in a swift stream. And if the view be extended to include the low degenerate forms, like the Ascidians, that are by some authors classed as chordates, the statement finds further emphasis.

"It is not difficult for the imagination to picture a lowly aggregate of animal cells, still plastic and indeterminate in organization, brought under the influence of a persistent current and caused to develop into determinate organization under its control, and hence to acquire, as its essential features, a spindle-like form, a lateral flexibility, and a set of longitudinal side-muscles adapted to rhythmical contraction, since these are but expressions of conformity and responsiveness to the shape and movement normally impressed by the controlling environment upon plastic bodies immersed in it. The necessity for a stiffened axial tract to resist the longitudinal contractions of the side muscles and thus to prevent shortening without seriously interfering with lateral flexibility, is obvious and is supplied by a notochord. Thus, by hypothesis, the primitive chordate form may be regarded as specific response to the special environment that dominated the evolution of a previously indeterminate ancestral form.

"In this larger application of the interpretation herein suggested, the chordate phylum is made to be essentially from first

to last a terrestrial race, whose main habitat was the land waters and the land itself, though still a race that sent its offshoots down to sea from time to time from the mid-Paleozoic onwards."

These five paragraphs constitute the most hypothetical portion of the article, and that the hypothetical element was large was duly recognized by Chamberlin who was careful to state that the problem at that time admitted of no other than hypothetical treatment. Thirty years have since lapsed and the problem is essentially where Chamberlin left it. But the original article and the textbook exemplification of it have been notable contributions to one of the big problems of the evolution of living forms.

The textbook was the fruition of many years of research and teaching experience. Both authors were teachers of exceptional power and effectiveness. Chamberlin used the Socratic method in earlier years and with the occasional undergraduate class which he continued to teach nearly to the appearance of the textbooks; his later classes were exclusively graduate and to these he lectured, with an impressive command of the subject matter, upon the principles and theories of geology. Salisbury followed the Socratic method almost exclusively, and was recognized as one of the most outstanding teachers in the University. He was nearly at his best when the textbooks appeared. So the three-volume "Geology" served to extend widely the classroom spirit and intellectual methods of its authors, and through these, as well as by the great original contributions within its pages, it helped notably to mould American geologic thought.

THE MISSION TO CHINA

With that far-reaching vision which characterized so many of his benevolences, Mr. John D. Rockefeller conceived the idea of doing something worthwhile for the Chinese people. A sum, adequate for a commission to visit China, reconnoiter the ground, and prepare recommendations, was entrusted to the University of Chicago. The University's choice of commissioners to conduct this survey of China and its needs fell upon

Ernest D. Burton and T. C. Chamberlin—two men selected from widely different spheres of thought to combine their humanistic and scientific experience in a balanced team. Horace G. Reed and Rollin T. Chamberlin were appointed assistants. Professor Burton and his assistant encircled the globe traveling eastward and studying conditions in India en route, while the Chamberlins, to bring a different point of view, went westward, the whole party assembling in Shanghai at the beginning of February, 1909. For nearly five months thereafter the commission traversed China by rail, river boats, sedan chairs, and Peking carts, studying fifteen of the eighteen provinces of China proper and making short excursions into Mongolia and Manchuria.

Chamberlin was profoundly impressed by the industry, thrift, and practical good sense of the Chinese people, who, in spite of strange superstitions and weak civic and nationalistic spirit, have steadily maintained their civilization for forty centuries during which many an oriental and western civilization has budded, flowered, and gone into decay. Entering into the project in hand with great enthusiasm, many a day on the march and after dark before the flickering flame of a tiny nut-oil lamp in some rustic Chinese inn, he unfolded to his companions the dreams and plans which came to his fertile mind. He drew lines for railroads, visioned in detail more scientific utilization of natural resources, mineral and agricultural, grappled with the application of western medical practice and research to the conservation of human life, and thrashed over the many possible ways of blending eastern and western educational methods and institutions so as to yield the best prospective results in the Chinese environment. His idea throughout was to graft, as far as possible, improvements and innovations on what already existed and to plant anew as little as possible, thus avoiding the semblance of revolutionary changes.

The commission visited the various viceroys and other high government officials to discover their general attitude toward foreign cooperation, sought their opinions and suggestions, and established good working relations with Chinese officialdom. Government schools were carefully investigated. The foreign

mission schools and missionaries contributed, on their side, the practical experience and conclusions growing out of many years of work and struggle with Chinese problems. Chinese and foreign views were quite diverse, but together they disclosed fairly the needs and opportunities, the strong and the weak sides of the Chinese people and institutions, and the desirable and undesirable forces and tendencies then in operation in China.

But Chamberlin was too highly original a man to rely principally on the opinions of others when given carte blanche to make recommendations for something new of large proportions whose potential possibilities depended in considerable part on the judgment and constructive ideas of the commission. The commissioners must see China with their own eyes and in the final analysis build chiefly on their own observations.

Their climacteric excursion was up the Yangtze Kiang into far western China to the borders of the great mountains which buttress Tibet on the east. For thirty-nine nights, going and coming, Chamberlin slept afloat this great river and its tributary, the Min, mostly in native house-boats. The stupendous gorges above Ichang, where the river has cut through a folded mountain range, claimed a full week on the up journey in spite of favorable winds and a bonus to the boatmen for their best speed. Exposure of the whole Paleozoic stratigraphic section of central China in the canyon walls, with glacial tillite at the base of the Cambrian system, however, compensated the geologist for this enforced vacation from educational investigation.

Subsequently, four hundred miles in fourteen days across the Red Basin of Szechuan in sedan chairs took the commission through one of the loveliest garden spots on the face of the globe. Though sixty-five years of age, Chamberlin did much of this on foot for more intimate contact with the country and the people. The marvelous terracing of the hillsides, the intensive cultivation of the soil with utilization of plant reciprocities to secure the simultaneous cooperation of plants well fitted to one another so as to yield the farmer three to five crops a year, the simple but clever inventions of the Chinese, and the never-failing picturesqueness of the daily scenes, were a constant

source of delight to him. Here was an old civilization unaffected by influences from without. Its inherent strength was plainly manifest, yet disease was rampant and dread epidemics swept away droves of people from time to time. In sanitation and in medical practice these people were still in the Dark Ages. The prompt cures by even such simple antiseptics as the members of the commission applied to the festering wounds and sores of several of their chair-bearers and boatmen astonished the natives. The white man's magic was plainly needed.

The commission eventually submitted a report of 977 pages covering the general outlook, the educational needs and the opportunities for large philanthropic work in China. The outcome of the report was a decision to concentrate effort in the field of medical science. The splendid Peking Union Medical College of the Rockefeller Foundation is now the tangible expression of this decision.

Their mission in China accomplished, the Chamberlins entrained at Mukden for Moscow in June, 1909. Ten days on the trans-Siberian train gave a much-needed rest, but the months of primitive travel and poor food had taken their toll. The first symptoms of serious stomach trouble, which was to afflict Chamberlin from time to time through the remainder of his life, now appeared. Slowly forming cataracts had already commenced to dim and blur his vision. A projected trip into Turkestan had to be abandoned, but father and son then zigzagged over much of Europe from the North Cape of Norway to Italy, Hungary and Roumania on the south, and from the Urals to the Atlantic, crossing as many of the mountain ranges as they could for a general picture of the orography and tectonics of Europe.

The expedition to China marked the final stage of Chamberlin's active field work. Well over six feet in height, of large frame which carried more than two hundred pounds, he was powerful physically, and the vigor which he displayed in tramping the paved paths of China seemed to show that he had lost little of his physical prowess with the advance in years. He felt fit and thought little of fatigue. But the break in health which began

unexpectedly on the return across Siberia with stomach ulceration and severe attacks of sciatica, gradually grew worse and at several times during the next three or four years his condition occasioned alarm. Eventually skillful treatment prevailed and during the last fifteen years of his life his health slowly improved except for steadily failing eyesight.

PHILOSOPHY OF CORRELATION

Our knowledge of the history of the earth has grown gradually by fitting together the local histories of small areas studied in detail, but as yet the general story is very incomplete. Most crucial and also most difficult has been the correlation of events in widely separated regions. An early advance in working method was the division of time into geologic periods based on major events of the earth's history. As the underlying reasons and basis for the division into geologic periods came to be better understood, the classification and correlation of events took on a new meaning. In 1874, J. S. Newberry had recognized the fact that the transgressions of oceanic waters on the continents and the resulting marine sedimentary accumulations have been cyclical in nature. In Volume I of the Geology of Wisconsin (1883) Chamberlin defined the typical geologic period to provide background for his treatment of the Paleozoic systems of Wisconsin, and presented a clear picture of the sequence of events which normally occurs during a period. "Ideally there might be said to be three main epochs in each period: (1) that of advancing waters and coarse detrital deposits; (2) that of deep waters and limestone deposits; and (3) that of retiring waters and mixed shaly deposits; but practically such ideal symmetry is usually broken by minor oscillations and irregularities of movement." [41]

Fifteen years later he took up the philosophy of correlation in the first of a long series of fundamental studies.

"The most vital problem before the general geologist today is the question whether the earth's history is naturally divided

[41] Geology of Wisconsin. Vol. I (1883), p. 210.

into periodic phases of world-wide prevalence, or whether it is but an aggregation of local events dependent upon local conditions uncontrolled by overmastering agencies of universal dominance. That there were no universal breaks in sedimentation or in the fundamental continuity of life is not only admitted but affirmed without hesitation . . . If, therefore, we seek for absolute divisions we doubtless seek in vain. But this does not dismiss the question whether the continuity of physical and vital action proceeded by heterogeneous impulses or by correlated pulsations. If the latter, then the history of the earth, when deciphered, will assume a rhythmical periodicity susceptible of natural classification and of significant and rational nomenclature; if the former, the contradictory phases of local actions will inhibit all but the most general unity and render classification and nomenclature either arbitrary or provincial." [42]

Chamberlin believed that the evidence strongly favored periodicity of the larger movements. The major movements of the earth's surface seemed to him to have been the sinking of the ocean bottoms and the withdrawal of the shallow epicontinental seas into the deepened basins. A case for the sinking was found in the force of gravity operating on the material of the earth's interior to produce rearrangement in more compact form. Quite different this was from the prevalent idea of contraction of the earth due to cooling from a molten condition. In the general process of rearrangement of matter in favor of greater density, the oceanic segments, being larger and of heavier material, sank most and the smaller and lighter continental segments sank less, thus increasing the topographic relief between the ocean basins and the continental protuberances. In the downcrowding process, the weaker, lighter continental segments were squeezed between the stronger, heavier oceanic segments and their borders in particular were wrinkled into mountain ranges.

Emergence and deformation of the continents in this manner take place with relative rapidity apparently when gradually accumulating stresses finally reach the yielding point of the materials. Then accompanying withdrawal of the shallow seas

[42] The Ulterior Basis of Time Divisions and the Classification of Geologic History. Jour. of Geol., Vol. VI (1898), pp. 449-50.

interrupts the marine stratigraphic record. Land areas become extensive and locally the topographic relief is high. Climates become diversified and plants and animals are subject to the stress of changed environmental conditions. The whole combination of interrelated events is a diastrophic revolution involving and affecting geologic and biologic processes to an exceptional degree. An old period has been brought to an end and a new one begins.

Erosive processes now attack the rejuvenated lands with maximum vigor and in time reduce them to lowlands and extensive sea-level plains. Sands and muds carried to the ocean displace their volume of sea water. Sea level rises and shallow waters advance widely upon the bevelled land surfaces. Extensive sedimentary deposits accumulate on the continents constituting a geologic rock system. Climates are mild and life in general thrives. Such conditions develop slowly and continue for a long time. Eventually, growing stresses within the earth bring on another diastrophic revolution; the geologic period is ended, and another cycle is inaugurated.

So basic are these principles in fundamental geologic philosophy, that Chamberlin's own words may be repeated for further exemplification.[43]

"I trust that many of you will agree that, in general, the relatively upward movements of diastrophism have been located continuously in the continents, and the broad downward movements continuously in the ocean basins, and that, setting aside incidental features, the dominant effect of the successive diastrophic movements has been to restore the capacity of the ocean basins and to rejuvenate the continents. This conclusion seems to me to be strongly supported by the general course of geologic history, wherein sea-transgressions and sea-withdrawals have constituted master features. Perhaps our firmest ground for this conviction is found in the present relations of the continents and the sea basins. If heterogeneity had dominated continental action in the great Tertiary diastrophisms, the results should stand clearly forth today. Some continents should show recent general emergence, while others should show simultaneous general

[43] Diastrophism as the Ultimate Basis of Correlation. Jour. of Geol., Vol. XVII (1909), pp. 688-92.

submergence. The dominant processes today should be those of depressional progress, on the one hand, and those of ascensional progress, on the other. As a matter of fact, all the continents are strikingly alike in their general physiographic attitude toward the sea. They are all surrounded by a border-belt, overflowed by the sea to the nearly uniform depth of 100 fathoms. These submerged tracts are all crossed by channels, implying a recent emergent state. None of the continents is covered widely by recent marine deposits, and yet all show some measure of these. Wide recent transgressions in one part do not stand in contrast with great elevations in another. Even beyond what theory might lead us to expect, when we duly recognize the warping incidental to all adjustments, the recent relations of the continents to the seas conform to one type. The 10,000,000 square miles of continental margin, now submerged, is distributed around the borders of all the continents with a fair degree of equability. May we not, therefore, agree that in the world-wide phases of diastrophic movements, the basins have been additionally depressed and the continents repeatedly rejuvenated.

"It is important that we should agree, or agree to disagree, on one further point. Have diastrophic movements been in progress constantly, or at intervals only, with quiescent periods between? Are they perpetual or periodic? The latter view prevails, I think, among American geologists. This view has acquired special claims since base-leveling has come to play so large a part in our science, for it is clear that the doctrine of base-leveling is specifically inconsistent with the doctrine of perpetual deformation, for the very conditions prerequisite to the accomplishment of base-leveling involve a high degree of stability through a long period. The great base-levelings, and the great sea-transgressions, which I think are little more than alternative expressions for the same thing, have, as their fundamental assumption, a sufficient stability of the surface to permit base-leveling to accomplish its ends. Shall we not therefore agree that there has been periodicity in the world-warping deformations? Let this not be held with such exclusiveness that we fail to recognize duly the effects of the adjustment of minor stresses, at other times. These may be preliminary of after-effects of the larger movements, or they may be due to local stresses more or less independent of the general body-stresses. These quite certainly have been present, and have produced intercurrent departures from the strict tenor of the great systematic movements.

"Correlation by general diastrophic movements takes cognizance of four stages: (1) the stages of climacteric base-leveling and sea-transgression, (2) the stages of retreat which are the first stages of diastrophic movement after the quiescent period, (3) the stages of climacteric diastrophism and of greatest sea-retreat, and (4) the stages of early quiescence, progressive degradation, and sea-advance.

"(1) The characteristics of the climacteric stage of base-leveling and sea-transgression need little further characterization here, for the function of base-levels is known to all American geologists and the function of great sea-transgression to every stratigrapher and paleontologist. We have in base-leveling conjoined with sea-transgression, just that combination of agencies which is competent to develop the broad epicontinental seas of nearly uniform depth requisite for an expansional evolution of shallow-water life. At the same time, it furnishes for wide migrations and the comminglings that lead to cosmopolitan faunas of the shallow-water type.

"(2) The stages of initial diastrophism and sea-retreat find their criteria in the deposits that spring from an increased erosion of the deep soil-mantles accumulated in the base-level period, in the effects of increasing turbidity, in the lessening areas suitable for the shallow-water life, and in the limitation of migration.

"(3) The climacteric stages of diastrophism are marked by the stress of restrictional evolution among the shallow-water species; by increased clastic deposition in land basins, on low slopes, and on sea borders, by great land extension, but often, perhaps dominantly, by diversity of land surface and by liability to climatic severities and diversification. Arealy, land life is favored, but it is hampered by the climatic and topographic diversities, and these may prove graver obstacles to migration and intermingling than even the tongues of sea that previously traversed the land surface. Correlation by glaciation in these stages is likely to prove a valuable adjunct, but we must first test our criterion, for we are not as yet quite sure that contemporaneity of glaciation is inferred on reliable grounds. The shallow-water life of the diastrophic stages is driven into narrow border tracts and into local embayments, and is thus forced into special adaptations and into narrowly provincial aspects.

"(4) The early stages of quiescence and of base-leveling, with advancing seas, are peculiarly fruitful in biological criteria, for they are marked by re-expansions of the narrowly provincial shallow-water faunas of the previous stages. The progressive

development of these provincial faunas and their successive unions with the faunas of neighboring provinces, as these come to coalesce by means of the progressive sea-advances, form one of the most fascinating chapters in life evolution, and give some of the most delicate of criteria for correlation.

"We are accustomed to look to the life record as our chief means of correlation. Its very high utility is quite beyond discussion. Thoughtful students, however, recognize that the paleontological record is based, in an essential way, on stratigraphy and that it is corrected and authenticated by the precise place the life is found to occupy in the stratigraphical succession. Stratigraphy and paleontology thus go hand in hand, each sanctioning the other. *Diastrophism lies back of both and furnishes the conditions on which they depend.*"

Hence Chamberlin's dictum: Diastrophism is the ultimate basis of classification and correlation.

THE ORIGIN OF LIFE

A radically new story of the parentage and birth of the earth having appeared in the planetesimal hypothesis, a new interpretation of its juvenile and mature activities was required. To this and to the working out of a comprehensive geologic philosophy on the new lines, Chamberlin devoted his later years. The newly opened leads naturally carried him into many different fields, one of which was the beginning of life on the earth.

An earth built up by accretion of planetesimals naturally presented very different surface conditions from one solidifying from a molten state. Instead of being entirely of crystalline igneous rock, the surface material of the youthful earth on the new basis was in part igneous, in part sedimentary and in part a heterogeneous assemblage of planetesimal matter of composition not radically different from that of the meteorites falling in today. This was the starting point. Rather opportunely an investigation of the gases obtained from heating meteorites in vacuo made during a larger study of the gases in rocks,[44]

[44] Rollin T. Chamberlin, The Gases in Rocks. Publication No. 106, Carnegie Institution of Washington (1908), p. 80.

brought forth some facts seemingly of much significance. Meteorites were known to contain carbides, nitrides, sulphides, phosphides, and hydrocarbons, and it was noted both that these peculiar compounds are unstable on the surface of the earth, and that they include the particular elements which comprise living tissue and are essential to life. Furthermore, these unstable compounds of the elements essential for life were, according to the planetesimal hypothesis, commonly intergrown, or otherwise closely associated with one another, so that the products of any reactions that might grow out of their instability would be favorably located for reaction upon one another. Particularly significant, therefore, was the close intergrowth of these compounds of the organic elements in states of combination which, while stable in the cosmic regions surrounding the earth from which the planetesimal material was gathered, were pronouncedly unstable in the presence of air and water, even at the ordinary temperatures at which organic synthesis takes place. On the earth these compounds would inevitably have participated in chemical reactions giving rise to other combinations of the elements critical to biologic activity.[45]

With his characteristic thoroughness of treatment of problems, Chamberlin proceeded to analyze the environmental conditions that would have favored organic synthesis on the youthful earth. The open ocean seemed to him unfavorable for the start of life, for there diffusion and dilution of the products of reactions would prevail. But on the other hand, the porous mantle of the land areas, supplied by planetesimal infall with unstable carbides, nitrides, phosphides, and sulphides steadily undergoing transformation into more stable compounds and generating during this process hydrocarbons, ammonia, hydrogen phosphide and hydrogen sulphide gases mingled with the ordinary gases carried by the planetesimals, furnished exceptional opportunities for interactions and combinations.

[45] T. C. Chamberlin and R. T. Chamberlin, Early Terrestrial Conditions that may have favored Organic Synthesis. Science, Vol. XXVIII (1908), pp. 897-911.

For specific illustration, Chamberlin selected a porous soil mantle situated alongside of a permanent body of water of appropriate salinity. In such an environment he envisioned many natural processes likely to cooperate in synthetic activity. Among these were percolation of water conditioned by a fluctuating water table due to accessions and evaporation, capillary action, osmosis, catalytic action, polymerization and other possible activities. Possible rôles which might be played by these were portrayed tentatively to deploy the problems in detail, but with constant reminders that nothing more than hypothetical suggestion was in mind.

What was attempted, however, was to indicate that in the early stages of the earth's development, according to the planetesimal hypothesis, the appropriate chemical elements, predisposed to enter into new combinations, were brought together in environments and under conditions singularly favorable for organic synthesis in the direction of primitive life. It was shown also that later, when planetesimal infall slackened, the opportunities for inception of life in this manner were greatly reduced. Chamberlin believed that the geologist's proper contribution to the problem of life beginnings on the earth was to handle the available stage settings for the start of the life story. This seemed to be accomplished by presenting a combination of inorganic processes operating on the necessary materials under conditions of such exceptional nature that they might have led into the realm of the organic. But to supply the vital spark of life which actually started the drama of life and kept it going falls naturally into the province of the biologist or the bio-chemist.

THE ORIGIN OF THE EARTH

When the planetesimal hypothesis was about fifteen years old and had become widely known from the textbooks and technical articles by its two authors, Chamberlin yielded to the request for a less technical presentation of the hypothesis in book form. The outcome was his little book, "The Origin of the Earth,"

which was published in 1916. To tell the story simply but well was his endeavor. For a better understanding and a more intimate contact, he took his readers into his workshop to show them the tools with which he had worked, and how, step by step, the investigation had been carried on. By relating how some efforts had proved futile and how various paths had led to no useful results he revealed the fullness of his explorations, unproductive as well as productive, and the persistency of his search for the truth.

"The Origin of the Earth" typifies Chamberlin's manner of unfolding a theme to the serious lay reader. The treatment throughout is direct and forceful, but the wealth of ideas and the rich literary style make the book far from light reading for either the scientific or the linguistic student. The reader must take it at its own level, for the author never allowed himself to lower his standards, though he simplified as much as possible by avoiding technical terminology.

The evolution of the planetary system and the juvenile shaping of the earth and its reorganization may be considered the key topics of the volume. The first is the planetesimal hypothesis, already considered; the others are the earth's growth and youthful development, worked out in accordance with that hypothesis and the state of geologic knowledge at the time. The infall of planetesimals, the holding of an atmosphere close about the young earth, and the gathering of its primitive waters into its surface hollows were the three concurrent activities which gave direction to the planet's growth and thus dominated its later career. The cooperation of this triumvirate—the lithosphere, hydrosphere and atmosphere—in the progressive shaping of the earth's surface is an intricate story partly told in this book but more fully developed in later publications.[46]

Changes in the rate of rotation, as the planet grew by adding to itself the planetesimals, set up stresses which analysis seemed

[46] Study of Fundamental Problems of Geology: The Growth of the Earth. Carnegie Institution of Washington, Yearbook No. 25 (1926), pp. 372-87.

to show might lead to a primary segmentation of the earth body. Modifying atmospheric and hydrospheric influences also came into play. The finer and lighter planetesimal materials floated longest in the atmosphere and were brought down by rain and snow especially in the regions of greatest precipitation. These being dominantly the land areas, judged by analogy with present precipitation, such areas gradually came to be built up of materials slightly less in specific gravity than those falling on the water areas. Slight though these differences in specific gravity were, they were perpetuated and increased by selective action of the hydrosphere. The lands suffered weathering and wash, and the dissolved portions were added to the oceans, either to remain in solution in their waters, or to be deposited on their bottoms. The residual material left on land was richer in silica and aluminum and consequently of lower specific gravity than the more soluble compounds of calcium, magnesium, sodium, and potassium which found their way to the sea. Small as were the differences in density thus brought about, the separating processes were long in operation during the later growing stages of the earth, and tended to develop sub-oceanic segments of greater density than continental segments. Reorganization of the material, local liquefaction, and magmatic segregation proceeded in their appropriate ways in the natural course of events. In subsequent deformations of the globe the heavier oceanic segments were especially prone to sinking, thus accentuating the ocean basins and leaving the lighter continental segments standing progressively higher and more sharply differentiated. In this way the foundations were prepared for the ensuing geologic story.

DIASTROPHISM AND THE FORMATIVE PROCESSES

The autumnal years of Chamberlin's life were given in considerable part to gathering in some of the slowly ripening fruits of the planetesimal hypothesis. The most systematic harvesting consisted of a series of fifteen articles under the heading, "Diastrophism and the Formative Processes," which appeared in the *Journal of Geology* between 1913 and 1921. The planet-

esimal cosmogony had opened the way for the evolution of an elastico-solid earth, though it did not exclude the possibility of a molten earth or even the probability that molten and gaseous states may dominate planets much more massive than the earth. "The long, slow growth of the main mass of the planet offers rather strong presumption of a relatively cool, solid accretion attended by heterogeneities of composition and differentiations of accession and crystalline organization that were never smoothed out by liquefaction, but have remained of the same type as those now presented by the earth." The concept of an elastico-solid earth implied an organization and subsequent development very different from that of the liquid globe based on the Laplacian hypothesis. To Chamberlin this concept seemed to avoid many embarrassing difficulties with which he had struggled for years and, as he tested the different possibilities, it was gratifying to find that the new lines grew increasingly fruitful in leading to further discoveries.

Shrinkage of a cooling earth had been found to be inadequate to produce the surface corrugations observed in the many belts of mountain folding. And yet, following the molten globe hypothesis, about all the shrinkage left to be registered in the rocks would have been the meager amount that might have resulted from cooling for, with the passage of the earth substance from the assigned gaseous to the liquid state and thence at length to the solidifying state, a large part of all possible adaptation to the demands of pressure should have taken place before the record of disastrophism began. Furthermore, cooling would have been opposed by an embarrassing supply of heat from radioactive processes.

On the other hand, an earth built up slowly by the accession of solid particles would have retained very nearly its maximum resources of combination, adjustment, and compression. Such an earth was potentially capable of great compacting and consequent deformation. Chamberlin sought the probable order of magnitude of the shrinkage of the earth by comparing our planet with neighboring members of the solar system. The

moon, whose mean radius is 1080 miles, has an average specific gravity of 3.34; Mars, whose radius is 2170 miles, has a specific gravity of 3.58; Venus, whose radius is 3851 miles, a specific gravtiy of about 4.85; and the earth, whose radius is 3959 miles, a specific gravity of 5.53. If these four bodies were built up by the ingathering of planetesimal material of essentially the same density, and if the progressive increase of density with increasing size of the body be the result of greater compaction under greater pressures, the shrinkage in volume may readily be computed. Chamberlin found that an earth built up of moon material of density 3.34, if changed to the earth's density of 5.53 by rearrangement of matter under pressure, would have undergone a circumferential reduction of 4555 miles, corresponding to a radial shortening of 725 miles. Perhaps more surprising was the fact that in changing from the size and density of Venus, a stage in which 80 per cent of the growth of the earth had already been attained, to that of the present earth a circumferential shrinkage of 1112 miles would have resulted. Apparently it might reasonably be concluded that the rate of shrinkage per unit increase of mass was greatest in the final stages of the earth's growth.

Although some of the greater density of the earth may have been due to heavier planetesimal material than that which went into the moon and the other planets,[47] the amount of shrinkage deduced was so great as to appear ample for all the crustal shortening implied in the contorted rock formations. The planetesimal earth thus possessed adequate potentialities for crustal deformation. A basis for diastrophism was thereby provided though Chamberlin left the specific activities and modes of concentration for more complete analysis in the concluding article of the series: "The Self-Compression of the Earth as a Problem of Energy."

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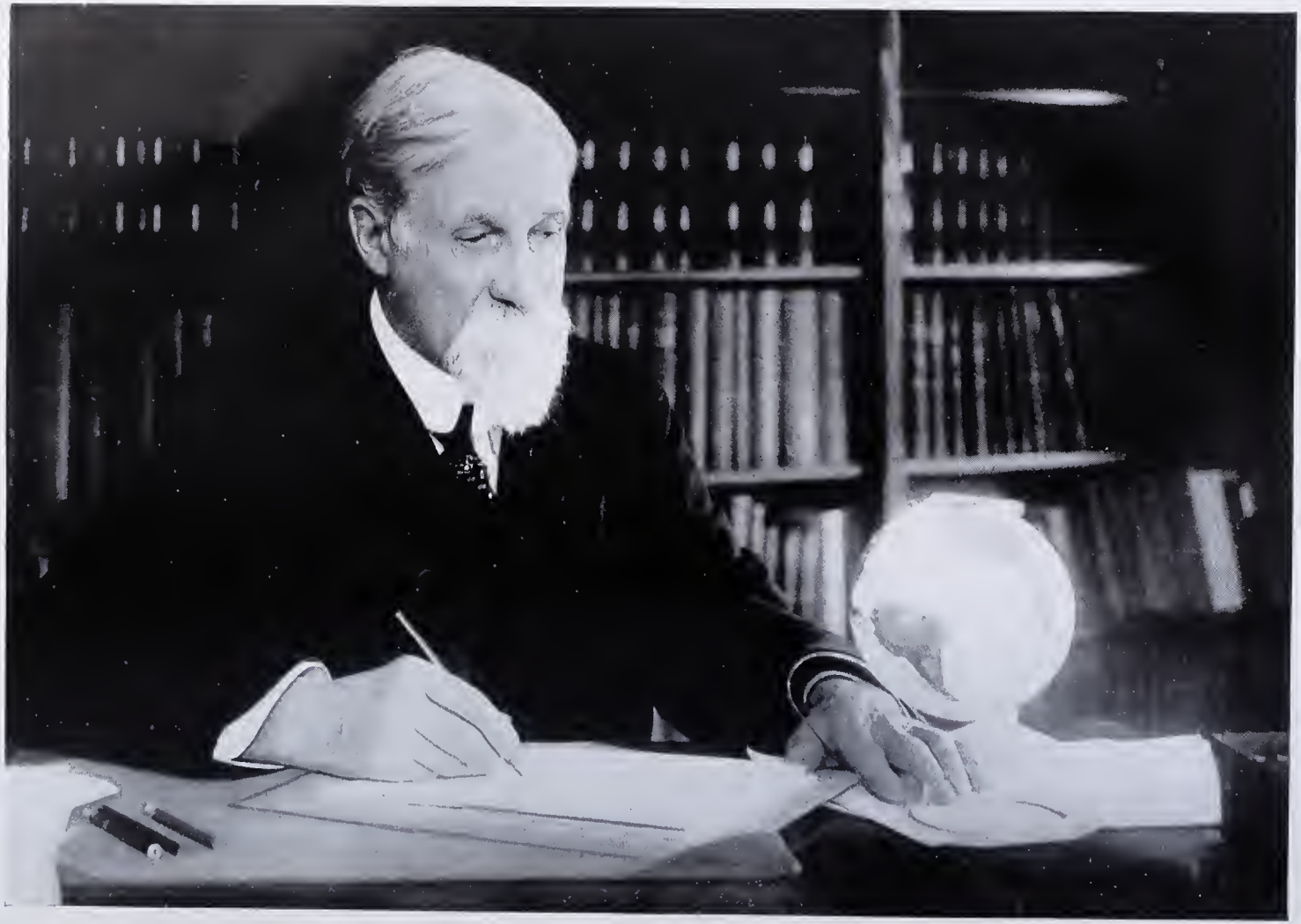
[47] Reasons, however, were advanced for thinking "that if the moon, Mars, Venus and the earth were built up normally by the planetesimal method, they should contain proportions of inherently heavy material in the inverse order of mass."

The series "Diastrophism and the Formative Processes" did not attempt a consecutive presentation of its author's philosophy of diastrophism. Many phases of that philosophy had been adequately presented before and some of them have already been considered briefly in this memoir. Rather was it a succession of critical essays on selected phases which Chamberlin wished to develop and extend beyond previous treatments. In particular, he gave much attention to the geologically significant interplay between diastrophism and the activities of the sea. The rejuvenation of the continents, their essential permanence as major relief features of the globe, the lateral stresses within the continental proturberances and their relations to continental creep and sea-transgression, the growth of the continental shelves, and the significance of the shelf-seas covering them, stood forth as subjects of prime consideration.

The geologic record shows that long periods of relative quiescence, during which the lands have been reduced to peneplains of wide expanse, have occurred from time to time. It is equally apparent that the base-leveling process cannot normally reach the stage of an advanced peneplain if continuous warping of the crust is in progress. On the contrary, the crust must hold an approximately static attitude long enough for the gradational processes to accomplish the results observed. The conclusion is clear that continental uplift has not been a continuous, steadily progressive process, but that it has recurred at such wide intervals as to permit in the meantime planation nearly to base-level with consequent evolution of shelf-seas and extensive inundations of the continental areas. All this seemed to Chamberlin to imply an elastic, rigid earth of sufficient strength to allow an accumulation of stress over a long period without notable yielding until, finally, easement of the stresses took place in a diastrophic revolution. The major diastrophic movements, according to the record, have been episodic in nature recurring between long periods of relative stability.

During times of stability, deposition of sediment on the outer slopes of the continental platforms built up continental shelves and extended them into the ocean basins. Chamberlin regarded



shelf-sea work as pre-eminently a process of terracing and the continents as pre-eminently the great terraces of the globe. Between the episodes of diastrophism the shelf-seas spread widely over the continents, extending the marine sedimentary deposits and affording opportunity for the evolution of shallow-water marine faunas. "The picture of the continents as essentially terraces wrought upon diastrophic embossments is no doubt the truest that can be found, and the contest between the diastrophic forces that emboss by protrusion and the gradational forces that terrace by planation and shelf-building, the chief physical battle of geologic history."

From the planetesimal hypothesis, followed logically through its natural chain of consequences under strict guidance from the testimony of the earth's past and present behavior, arose the foundations for a consistent, systematic interpretation of geologic history. Most fundamental was diastrophism for which the hypothesis afforded a ready cause and *modus operandi,* arising from the very nature of the postulated mode of the earth's growth. Chamberlin fitted in harmoniously the processes of vulcanism and made new contributions in that field. Cooperating with diastrophism in some ways and operating directly against it in others, the gradational processes played their appropriate part, as he brought out in masterly fashion. These in turn influenced earth climates in varying degree, and finally the whole combination, directly or indirectly, exerted a powerful control on the evolution of life.

The keynote of this series of articles was diastrophism in the large, for which Chamberlin coined the word, "megadiastrophism," to set it over the subsidiary manifestations of lesser magnitude ordinarily connoted by the term, "diastrophism." Few subjects have greater complexity or more ramifications, the mere enumeration of which reaches formidable proportions. To bring together some of the more essential developments and conclusions was Chamberlin's objective in the next to the last number of the sequence, which he called: "Groundwork for the Study of Megadiastrophism. Part I. Summary Statement of the Groundwork Already Laid." This was a succession of sev-

enty-seven separately numbered propositions or paragraphs, each one of which reads like the abstract of an independent paper. A vast field was tilled in these seventy-seven furrows, which the plowman felt he had merely seeded for valuable crops to come in the future.

THE TWO SOLAR FAMILIES

Chamberlin's last publication was his booklet "The Two Solar Families." This was started as a revision of "The Origin of the Earth" at the request of the University of Chicago Press. But he soon found that his ideas had developed so greatly since the appearance of the earlier volume that two books would be required to cover the ground at all adequately. In the first he proposed to delineate the origin of the solar system, and in the second to present the growth of the earth. The first he completed; the second remains unfinished.

"The Two Solar Families" is the story of the sun's children. In an introduction, scarcely surpassed for beauty in the whole realm of scientific writing, we are told that the progeny of the sun belong to two families—the Planetary Family, embracing the planets, planetoids, and satellites, and the Cometary Family, comprising the comets, chondrulites, and meteorites. One parent was common to both families; the other was the chief differentiating factor. A passing star, the planetary family's stellar father, called forth larger masses of sun substance than the sun was accustomed to give forth at other times and gave the planetary offspring relatively robust constitutions and orderly habits of behavior. The cometary family came from feebler cooperation on the part of the starry host outside the solar system and fell heir to too much of the turbulence and the fiery antagonisms of the sun, and to too little of the steadying orbital energies of the starry group outside. "The planets, planetoids and satellites are plain plodding children compared with the comets, the meteors, and the meteorites. But the lives of the latter seem to be as notably short as they are spectacular and reckless in the

wastage of their energies, while the lives of the former seem likely to be as long as they are modest, steady, and exemplary in the husbanding of their energies."

Among the changes in point of view from the earlier volume it is to be noted that the planetesimal hypothesis has become entirely divorced from the spiral nebulae which at one time had seemed to show family relationship. Much more important have become the spheres of activity, or spheres of control, of the sun and the individual planets, embracing both spheres of gravitative control and spheres of effective light pressure. The relative strengths of the pull of gravity and the push of light and other repellent forces of the sun distinguish the dynamic zones of the solar system. For small, light particles near the sun the repulsive forces overmatch the sun's gravitation and drive such particles away from the sun. Chamberlin saw in this a possible explanation for the striking fact that the four inner planets have grown less and are of higher specific gravity than the outer planets. He differentiated four dynamic zones concentric around the sun. The inner two he described as follows:

"I. The first constitutes the inner environment of the sun. It embraces the orbits of the small, naked or slightly clothed planets, Mercury, Venus, Earth, Mars, and the planetoids. The repellent agencies effectively drive out of this zone free molecules and fine dust, except as they are held by the superior attractions of Venus, Earth, and Mars. Comet's heads suffer wastage and depletion while traversing this zone. Tail-stuff is developed from comet heads and driven out of this zone with great velocity. At the same time it seems to have been possible for small planets, planetoids, and satellites to assemble, to survive their 'perils of infancy,' to grow to their present states, and to give promise of living on to unknown lengths in spite of all solar repellency. These planetary bodies even seem to have thrived under conditions that were hostile to the comets.

"II. Outside this inner environment of the sun lies the zone of the great gaseous planets, Jupiter, Saturn, Uranus, and Neptune. In this zone the selective solar repellency seems to be much abated. There seems to be greater tolerance of gases, and there certainly was greater gaseous growth."

To accord with the observed natural division of the planets into two groups, an outer one of four large low-density planets and an inner one of four small high-density planets, Chamberlin suggested that the large outer planets arose from projectiles shot toward the passing star and that the smaller inner planets arose from projectiles shot in the opposite direction from the remote side of the sun. These two groups of four bolts each he supposed to represent the systematic response of the sun to the differential attraction of the star as it passed obliquely over the sun's two specially eruptive belts on either side of its equator. "It is assumed that there were two shots toward and two shots from the star in crossing each belt, making, in all, four great missiles sent far out past the star in its rear and four smaller missiles shot shorter distances from the opposite belts on the far side of the sun. We thus postulate only four main double shots. The planetoids are supposed to have arisen from small irregular projections stimulated on the far side of the sun by the passing star before it reached the specially eruptive equatorial belts." Chamberlin believed that there should have been counterpart eruptions of this irregular type from the side of the sun toward the star, and these he thought might possibly be represented by undiscovered bodies beyond Neptune. In fact, he introduced a diagram[48] which includes several lines of projection (with a question mark) for planetoids beyond the orbit of Neptune. As is well known, the planetoid Pluto, revolving far outside the orbit of Neptune, was discovered two years after his death.

The later pages of "The Two Solar Families" are filled with such a wealth of new contributions that nothing more than mere citations of a few lines of advance can be attempted here. A very careful analysis of the presumptive activities of the planetesimals brought out more concretely and completely than ever done before their probable behavior and showed how very naturally they may have occasioned the peculiarities observed in the different members of the solar system. Particularly suggestive is the chapter on the growth-creep of the axes of the planets in

[48] The Two Solar Families (1928), Fig. 28, p. 156.

which it is shown that the impact of the planetesimals would shift the axis of rotation of a growing planet from the plane of projection of the solar outburst progressively toward a position at right angles to the plane of the solar system. The hitherto baffling inclination of the earth's axis, responsible so happily for our yearly seasons, thus apparently finds a very simple and plausible explanation. The searching analysis developed also possible explanations of the outstanding peculiarities of Mercury and Venus, the reason why the moon constantly presents the same face to the earth, and the reason for the opposing land and water hemispheres of the earth.

Although the cometary family is characterized by loose organization, yet Chamberlin brought the chondrulites, comets, and meteorites together as related products of gaseous outbursts projected to the farthest reaches of the sun's sphere of control where inequalities in the attractions of the stars outside gave side-pulls sufficient to cause the assemblage of now solid particles to miss the sun on the return journey and to swing around it in greatly elongated ellipses. Condensations of the gaseous masses on their outward journey produced the tiny chondrulites characterized by radical and concentric structures like the hailstones in a storm cloud. The whole swarm of chondrulites constituted a comet; when the comet came back into the heated environs of the sun at perihelion occasional collisions between a few of the chondrulites resulted in welding a very small proportion of them together into chondrule aggregates, which are the meteorites. So inherently simple and natural is the whole picture that it has much to commend it though it is radically different from its author's earlier conception of the formation of comets and meteorites.

"The Two Solar Families" came from the press on Chamberlin's eighty-fifth birthday, September 25, 1928, and the first copy to be finished was hurried to him for a modest celebration in the afternoon. He planned to rest for a month and then to take up again the manuscript for the second volume, "The Growth of the Earth," on which considerable progress had already been made. But a month later he was ill. The splendid

mechanism which had worked so long and so well was worn out. He was taken to the University hospital, but nothing in the power of his medical colleagues could restore his weakening heart, and he slipped away peacefully on a still further journey into the unknown, November the fifteenth.

THE MAN HIMSELF

On the human side, Thomas Chrowder Chamberlin stood out nearly as distinctively as his works did in the field of science. Six feet one-and-a-half inches in height, of large, athletic build, he was a rugged and vigorous figure throughout most of his life. On the farm he had had a neighborhood reputation for pitching hay and for other occupations combining strength and skill; in college he excelled in sports, particularly football of the kind then being played. As a young geologist and administrator, his obvious physical strength and mental capacity, his open, friendly, sympathetic nature, and his marked abhorrence of all that was false and mean, led to his appointment to positions of greater and greater responsibility. As the years went by, his characteristic seriousness of mien, softened by a naturally kindly and serene facial expression came to give him an appearance of exceptional dignity. Later his dignity was enhanced by white hair and furrows of thought which gradually roughened his brow.

Most characteristic of the workings of Chamberlin's mind were his independence of thought and his intellectual integrity. In the bitter partisanship of the Civil War, he put in a good word, now and then, for the South. At the University of Wisconsin, whose previous presidents had been clergymen, he answered the sneering question: "Who will now preach the baccalaureate sermon?" by giving his own baccalaureate addresses, and so effectively as to create a profound impression by his originality of thought and his inspiring delivery. When satisfied that his policies were along the right lines, he was fearless in putting them into effect, though safely guided by a shrewd sense of what could be accomplished satisfactorily and what was inadvisable to attempt.

Intellectual honesty he placed highest, as was apparent in most of what he did. In all probability his consistent success in securing appropriations from the Wisconsin legislatures was due, in no small degree, to the confidence which he invariably inspired and to the transparent reasonableness of what he sought. Political sense he had, but he made little use of political ties and gave still less thought to personal popularity. In fact, as university president, he could scarcely be called popular in the ordinary meaning of the word, for he subordinated the personal and social aspects of his position to the carrying out of his ideas and the maintenance of his ideals in the administration and development of the University.

At the University of Chicago he was back again in the scientific work and environment most congenial to him. He brought together for the Department of Geology the strongest group which he could procure: Rollin D. Salisbury, in geographic geology; Joseph P. Iddings, in petrology; Richard A. F. Penrose, Jr., in economic geology; Charles R. Van Hise, as part-time professor of pre-Cambrian geology; Stuart Weller, in invertebrate paleontology; and, later, Samuel W. Williston, in vertebrate paleontology. In addition there were others on special appointments of shorter duration. This was indeed a remarkable assemblage of men who represented not only the best in their respective fields, but who were conspicuous also for exceptional personal qualities. Whenever the department came together in public it attracted attention; people commented on such a group of uniformly tall, handsome, dignified men. They reflected in a way the ideals of their leader, who was as thoughtful of superior manhood as of superior scientific ability and attainments. Likewise the associate editors of the *Journal of Geology,* selected from leading institutions in this country and abroad, were an imposing array of outstanding geologists. In the midst of such inspiring colleagues and associates, Chamberlin was in a position to do the best work of which he was capable.

During the first decade of the growth of the new University, Chamberlin taught some elementary courses as well as special graduate courses in glacial geology and the more advanced

phases of general geology. His commanding personality, mastery of precise diction, and versatile fertility of mind, gave high cultural value to his undergraduate instruction. Though his contacts with the undergraduates were somewhat limited, whenever he did come in touch with them, they felt strongly his wholesome and stimulating influence.

But it was the graduate students who profited most from his lectures and the contact with him. For many years he gave a series of courses under the elastic title of "Principles and Theories of Geology." These ran through the whole gamut from cosmogony to the geology of the present and its implications for the future, and in the aggregate covered his entire geologic philosophy. Each year, completely ignoring the outline of the previous year, he presented his material afresh in accordance with his latest views and the newest developments. Thus it became quite customary for students to repeat the courses, for not only was a new slant commonly given to old problems, but many new problems were constantly coming to the fore. Cosmic geology, at the outset, and Pleistocene glacial geology, at the close, were naturally favorite fields for particularly thorough treatment; but outside of the more technical phases of paleontology and petrology, the whole geologic province came under his general survey. He elaborated the planetesimal hypothesis and traced with great care its bearing upon continental evolution, diastrophism, vulcanism, climatology, and the resulting biologic responses. All in all there arose a comprehensive geologic philosophy. But the prime object of these courses was not so much to present a consistent geologic philosophy as to develop the critical powers of the students in attacking new problems and thus to prepare them for their own researches. The chief emphasis was placed on sound, judicial thinking.

Setting an example was ever a vital part of Chamberlin's advanced teaching. He unfolded the steps of progress in many past researches and discussed critically his own and other current studies to bring out the objectives, difficulties in the way, and the means of overcoming them. Various students were utilized

as helpers in the details of the larger studies, and thus they obtained first-hand acquaintance with his methods. In fact, the extraordinarily thorough drill in precise geologic thinking by Salisbury coupled with the inspiration for research from Chamberlin was in considerable measure responsible for the conspicuous success afterwards achieved by the impressive list of young geologists trained at Chicago when these two teachers were in their prime.

The quarter century 1892-1918 while Chamberlin was head of the Department of Geology at Chicago included his most productive years. In spite of multitudinous demands on his time, he was then engrossed primarily in his researches. Here lay his greatest interest; he took no vacations; research was play and any spare time was an opportunity not to be lost. But rarely did he drive himself; when in the mood he worked enthusiastically and effectively; when not in the mood for a particular problem, he read or attempted something else. While engaged on one of his books he once wrote fifty pages in one day, loafed the next, browsing in the library, and then wrote sixty-six pages on the following day. Though his large script filled pages quickly, the actual output of material was surprising in amount.

He wrote spontaneously and rapidly when handling new material, but such writings, fluent though they were, never reached the printer. They were rewritten and revised over and over again. It was amazing how much interlineation could be put on a page of seemingly satisfactory manuscript. Since his handwriting was peculiar and none too legible, the stenographer must have groaned under the constant slaughter of her copy. But he invariably kept at it until the final product satisfied his critical taste.

Somewhat similar was his habit of solving problems. With great fertility of imagination, he dreamed rapidly ahead along the various alternative lines of solution following the possible ramifications whithersoever they might lead. Later he cast aside the unpromising leads. Rejecting on the one hand what he termed the "method of colorless observation" as sterile, and on the other hand avoiding the "method of the ruling theory" as

dangerous and likely to block the road to further advances, he believed that the true scientific method was to encompass the problem in all its aspects and to treat impartially the various factors and alternative possibilities. This he called the "method of multiple working hypotheses." [49]

"Conscientiously followed, the method of the working hypothesis is an incalculable advance upon the method of the ruling theory, but it has some serious defects. One of these takes concrete form, as just noted, in the ease with which the hypothesis becomes a controlling idea. To avoid this grave danger, the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. It is thus in some measure protected against the radical defect of the two other methods. In developing the multiple hypotheses, the effort is to bring up into view every rational explanation of the phenomenon in hand and to develop every tenable hypothesis relative to its nature, cause, or origin, and to give to all of these as impartially as possible a working form and a due place in the investigation. The investigator thus becomes the parent of a family of hypotheses; and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted. Where some of the hypotheses have been already proposed and used, while others are the investigators' own creation, a natural difficulty arises; but the right use of the method requires the impartial adoption of all alike into the working family. The investigator thus at the outset puts himself in cordial sympathy and in parental relations (of adoption, if not of authorship) with every hypothesis that is at all applicable to the case under investigation. Having thus neutralized, so far as may be, the partialities of his emotional nature, he proceeds with a certain natural and enforced erectness of mental attitude to the inquiry, knowing well that some of his intellectual children (by birth or adoption) must needs perish before maturity, but yet with the hope that several of them may survive the ordeal of crucial research, since it often proves in the end that several agencies were conjoined in the production of the phenomena. Honors must often be divided between hypotheses. One of the superiorities of the multiple hypotheses as a working mode lies just here. In following a single hypothesis, the mind is biased

[49] Jour. of Geol., Vol. V (1897), pp. 843–45.

by the presumptions of its method toward a single explanatory conception. But an adequate explanation often involves the co-ordination of several causes. This is especially true when the research deals with a class of complicated phenomena naturally associated but not necessarily of the same origin and nature, as, for example, the Basement complex or the Pleistocene drift. Several agencies may participate not only but their proportions and importance may vary from instance to instance in the same field [sic]. The true explanation is therefore necessarily complex, and the elements of the complex are constantly varying. Such distributive explanations of phenomena are especially contemplated and encouraged by the method of multiple hypotheses and constitute one of its chief merits. For many reasons we are prone to refer phenomena to a single cause. It naturally follows that when we find an effective agency present, we are predisposed to be satisfied therewith. We are thus easily led to stop short of full results, sometimes short of the chief factors. The factor we find may not even be the dominant one, much less the full complement of agencies engaged in the accomplishment of the total phenomena under inquiry. The mooted question of the origin of the Great Lake basins may serve as an illustration. Several hypotheses have been urged by as many different students of the problem as the cause of these great excavations. All of these have been pressed with great force and with an admirable array of facts. Up to a certain point we are compelled to go with each advocate. It is practically demonstrable that these basins were river valleys antecedent to the glacial incursion. It is equally demonstrable that there was a blocking-up of outlets. We must conclude then that the present basins owe their origin in part to the pre-existence of river valleys and to the blocking-up of their outlets by drift. That there is a temptation to rest here, the history of the question shows. But on the other hand, it is demonstrable that these basins were occupied by great lobes of ice and were important channels of glacial movement. The leeward drift shows much material derived from their bottoms. We cannot therefore refuse assent to the doctrine that the basins owe something to glacial excavation. Still again it has been urged that the earth's crust beneath these basins was flexed downward by the weight of the ice load and contracted by its low temperature and that the basins owe something to crustal deformation. This third cause tallies with certain features not readily explained by the others. And still it is doubtful whether all these combined constitute an adequate explanation of the phenomena. Certain it is, at least, that the measure

of participation of each must be determined before a satisfactory elucidation can be reached. The full solution therefore involves not only the recognition of multiple participation but an estimate of the measure and mode of each participation. For this the simultaneous use of a full staff of working hypotheses is demanded. The method of the single working hypothesis or the predominant working hypothesis is incompetent."

These were the basic principles of his scientific life and characterized his serious thought in other fields as well as science. They were already strongly in evidence in his youthful work on the lead and zinc problem in Wisconsin and continued to dominate his most mature cosmogonic researches. "Probably at no time in his career as an investigator did Chamberlin more courageously apply the method of multiple hypotheses than when, as a veteran of eighty odd years, he undertook to review and revise his work on the origin of the earth."[50] "No one else knows so well as I do that his every instinct and the habits of a lifetime kept him from ever making the planetesimal hypothesis a 'ruling theory.' To his last days he never ceased to regard it as a hypothesis to be put to the test in every possible way, rather than as an expression of final truth. . . . How seldom have these precepts been followed in science or philosophy! How different in many cases the results would have been had they been followed! What need there is for them also in social and political sciences, in theology, and in all the varied interests of the human mind!"[51]

The serenity of mind requisite for the long-sustained and successful journeys into the unknown was made possible by a happy home life which fortified him for his manifold tasks. What greater blessing can a man have than a sympathetic and understanding wife? What is more helpful than to find home ever a haven of rest from the cares and disappointments of the day's struggles? The difficulties melt away in the peaceful quiet of the happy home and the morrow starts fresh and full of

[50] Bailey Willis, Dynamics is the Soul of the Problem. Jour. of Geol., Vol. XXXVII (1929), p. 362.

[51] F. R. Moulton, Thomas Chrowder Chamberlin as a Philosopher. Jour. of Geol., Vol. XXXVII (1929), pp. 372-73.

promise. In this all-important part of his life, Thomas Chamberlin was particularly blessed. Mrs. Chamberlin was indeed his ideal counterpart. She was home-loving, quiet and somewhat retiring by nature, but practical and capable and strong in the optimistic faith brought over from the previous pioneer generation. Though naturally proud of the achievements of her husband, successes or failures were not allowed to alter the cheerful, even tone of the family fireside. For well over half a century their life together was singularly harmonious and happy. When, at length in the summer of 1923, she was the first to answer the final call, it seemed as if he too, so dependent on her in many ways, might not long survive the parting. But her spirit continued with him in effect and he carried on for five years more.

It was always a source of satisfaction to the father that his son, the writer of this memorial, chose to follow in the same line of work. They had many things in common and their companionship was most congenial. From early childhood the father talked to the son as to a grown person and so spontaneously there developed a complete understanding between them. In later years of more philosophical trend, he outlined elaborately his inmost thoughts in many of the different realms into which the human mind penetrates. Screen after screen seemed to be lifted before long vistas into the hidden beyond. The inspiration was tremendous, and the poetic beauty of the vistas beyond description. But only the magic lamp of a master can illumine such portals; when the light is removed, one is left groping, though groping with a little more confidence for the visions which have been revealed.

At the close of the academic year in 1918, when approaching his seventy-fifth birthday, Chamberlin retired from the University. He continued, however, as senior editor of the *Journal of Geology,* but gradually lessened the load of outside demands on his strength. His time he now considered his own, to be utilized to the best advantage. Researches, as ambitious as any of former years, continued to fill his mind and to keep him conscious of how much he still had to do in the next few years.

He was working more slowly now. Cataracts had been dimming his vision gradually for a decade and all reading now had to be done through a large hand glass, but he toiled on without complaint. An operation to remove the opaque crystalline lenses had seemed the ultimate solution of the difficulty, but conditions did not reach the point where an operation was advisable and it was never attempted. Fortunately the eyes served for ten years more. His general health, which already had occasioned some alarm, now improved perceptibly. Needing more time for his work so badly, in his characteristic way he turned his power of analysis and experimentation upon his stomach problem and eventually developed a regimen of treatment and control which accomplished the desired results and carried him along.

Ten years for the continuation and rounding up of his researches were eventually allowed him, and approximately one-fifth of all his published papers appeared in this decade. A perusal of the bibliography will show their diverse scope and the breadth of interest which he still retained. All were mature productions, as might be expected, but some were as highly original as the inventions of youth. Even the final year brought little loss of mental vigor. Significant and fitting it was that his last publication, "The Two Solar Families," which came from the press on his eighty-fifth birthday, should be one of the finest of all his productions. A few days more and his busy life was over. Like the evening star in the mountains which shines, full of glory, undiminished till the end, his work continued to this brilliant close. A companion volume was in prospect, but the light was cut off too soon.

This continued devotion to duty was an expression of his belief in the value of his researches. Partly as the result of a successful life and partly because of his habitual thoroughness in following the method of multiple working hypotheses, he came to have great confidence in his judgment. If, at times, he seemed impatient of some other views which were different from his own, it was only because he felt that their sponsors had not considered the ramifications of the problems so carefully or so thoroughly as he had, and hence were less qualified to express

judgment. Filled with enthusiasm by his message he wished to deliver it in full.

He was deeply religious in the truest sense, and the call of duty came likewise from his belief in the honesty of the Cosmos, as he expressed it, and from his prophetic outlook on life and its proper place in the Universe. The long geologic retrospect gave a basis for peering into the future. For hundreds of millions of years balanced processes have kept our globe a fit abode for life. A regulated system with orderly development has prevailed. Life has progressed to higher and higher levels. What has characterized the past is likely to characterize the future and the end seemed to him far distant. The heights to which the human race may attain, if it but utilizes its intelligence and knowledge to the best advantage, are likely to be far beyond our liveliest imagination. He sometimes remarked that if there were only a few thousand years ahead, much of the satisfaction which he took in his work would be lacking. But as the presumption is in favor of ages to come as long as those of the past, he believed that whatever good one truly accomplishes, if the results are to carry on as constructive factors in an advance for millions of years, may in the aggregate prove to be of inestimable value. This was his optimistic dream; he laid his few bricks and mortar with genuine satisfaction, and confidently looked to others to build higher and better in the long time to come. Now his own little bit is done; may the future substantiate his inspiring prophecy.

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